

# **BIOLOGICAL SYSTEMATICS AND EVOLUTIONARY THEORY**

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# **BIOLOGICAL SYSTEMATICS AND EVOLUTIONARY THEORY**

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University of Pittsburgh, 2015

In this dissertation I examine the role of evolutionary theory in systematics (the science that discovers biodiversity). Following Darwin's revolution, systematists have aimed to reconstruct the past. My dissertation analyzes common but mistaken assumptions about sciences that reconstruct the past by tracing the assumptions to J.S. Mill. Drawing on Mill's contemporary, William Whewell, I critique Mill's assumptions and develop an alternative and more complete account of systematic inference as inference to the best explanation.

First, I analyze the inadequate view: that scientists use causal theories to hypothesize what past chains of events must have been, and then form hypotheses that identify segments of a network of events and causal transactions between events. This model assumes that scientists can identify events in the world by reference to neatly delineated properties, and that discovering causal laws is simply a matter of testing what regularities hold between events so delineated. Twentieth century philosophers of science tacitly adopted this assumption in otherwise distinct models of explanation. As Whewell pointed out in his critique of Mill, the problem with this assumption is that the delineation of events via properties is itself the hard part of science.

Drawing on Whewell's philosophy of science, and my work as a member of a team of systematists revising the genus *Bassaricyon*, I show how historical scientists avoid the problems of the inadequate view. Whewell's account of historical science and of consilience provide a better foothold for understanding systematics. Whewell's consilience describes the fit between a single hypothesis and lines of reasoning that draw on distinct conceptual structures.

My analysis clarifies the significance of two revolutions in systematics. Whereas pre-Darwinian systematists used consilience as an evidentiary criterion without explicit justification, after Darwin's revolution

consilience can be understood as a form of inference to the best explanation. I show that the adoption of Hennig's phylogenetic framework formalized methodological principles at the core of Whewell's philosophy of historical science. I conclude by showing how two challenges that are frequently pressed against inference to the best explanation are met in the context of phylogenetic inference.

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## 1.0 INTRODUCTION

In this dissertation I examine the role of evolutionary theory in systematics (the science that discovers and studies biodiversity) by analyzing systematics qua historical science. Following Darwin's revolution, systematists have aimed to reconstruct the past. I analyze historical science, focusing on historical causal reasoning, in order to show the relationship between evidence and hypotheses about what the past must have been like. I explicate the debate between John Stuart Mill and William Whewell as it applies to these philosophical questions. Key assumptions of these philosophers framed subsequent understanding of historical science. Armed with this philosophical framework, I analyze the conceptual foundations of modern systematics, as developed by Willi Hennig, and explicate inference in systematics through consideration of a recent phylogenetic study.

The dissertation concerns the following questions: what is the status of historical science? How do historical hypotheses express causal information, and how does historical inference rely on causal reasoning? How do systematic methods reflect the historical nature of the science; how did systematics become a historical science? How do systematists take account of evidence? How do biological theories inform phylogenetic inference? How does the inter-theoretic nature of systematics play out in phylogenetic inference and in systematic hypotheses?

There is a tendency to dismiss pre-Darwinian systematics as unscientific. It might be wondered what could possibly be gained through consideration of pre-Darwinian philosophy of systematics. In fact, pre-Darwinian systematics as ahistorical science provides an invaluable resource for understanding what it means that systematics is essentially historical. Systematics is an ideal subject for understanding historical scientific methodology precisely because we can study the introduction of historicity to the science.

Historiography of systematics (and biology more generally) is still recovering from what Amundson (2005) calls the Typological Essentialism Story (TE-Story). The rejection of the TE-Story is now itself hardening so that the good work of TE-Story architects and proponents tends to get overlooked. Amundson, Winsor (2003), and others are correct that Darwin did not overthrow “2000 years of stasis” (Hull, 1965). Yet the introduction of evolutionary thought to systematics was absolutely transformational and required the identification of particular methodological problems. Analysis of historical scientific methodology thus promises to revise narratives about the history of systematics. One lesson from the TE-Story debates has been the extent to which historical narratives can be shaped and employed by modern biologists and philosophers of biology for the purpose of intra-disciplinary war. In this dissertation I develop my philosophical analysis of systematics with an eye to approaching historical systematists on their own terms.

I approach my historical philosophical actors through their own contexts and debates, because this is the only way to understand their claims. The purpose of my interrogation is to draw philosophical lessons for my own analysis of historical science. I engage with Whewell and Mill as philosophers. I also handle them as historical figures crucial to my history of philosophy of science: their views are carried forward in the debates that I examine.

From Mill (chapter two), I explicate assumptions about causal ontology and scientific explanation that would prove influential in subsequent philosophy of science. I also show that Mill held views on natural kinds that were completely at odds with his views on scientific laws and explanation in general.

Whewell provides a forceful critique of Mill’s assumptions (chapter three). I explicate the critique through consideration of Whewell’s alternate account of causal reasoning in science. Systematics appears to have been a major source for Whewell’s development of the concept of

consilience, which would prove influential for philosophical accounts of inference to the best explanation.

I argue that Mill's assumptions underlay twentieth century debates on explanation (chapter four), which took a cyclical form because of failure to root out the assumptions as the source of the relevance problem. The explanation debates in turn framed the debate on the status of historical science, and the nature of historical hypotheses and explanations. My task is to trace the role of Mill's assumptions in philosophy of science, showing how the assumptions contributed to misunderstanding of historical science.

From Mill's views on natural kinds I explicate an account of historical causal dependence (chapter five) in order to analyze how historical hypotheses convey causal information. My account draws heavily on Whewell's philosophy of historical science and in particular his arguments about what historical theories convey. In the course of this account I describe Whewell's own foray into the practice of historical science, which has been completely overlooked in the secondary literature. My philosophical account of historical hypotheses clarifies how the adoption of a phylogenetic system render systematics essentially historical. I explain this system through Willi Hennig's (1966) *Phylogenetic Systematics*, a work which provides a particularly clear exposition of the conceptual framework of modern systematics.

In chapter six I analyze phylogenetic inference as inference to the best explanation. Phylogenetic methodology provides a response to the bad lot objection (B. Van Fraassen, 1980) and clarifies the tautology objection (Lipton, 2004) to the adequacy of inference to the best explanation. Consideration of systematics shows that a related problem, the problem of evidence, has not been sufficiently addressed in philosophical accounts of abductive inference.

Because the first chapters launch directly into the Mill/Whewell debate, some brief historical and historiographical context may be helpful. Laura Snyder (2006, 2011) has provided the most

comprehensive recent account of the debate. One of her foremost concerns is to establish the importance of the debate in the development of both Mill and Whewell's philosophies of science. An understanding of the debate is critical to understanding either Mill or Whewell. I agree. As will be seen, the points of dispute between Mill and Whewell in regard to causal reasoning in science are at the heart of their competing systems. These points are crucial to understanding the philosophical assumptions that descend from each philosopher. Snyder was also much concerned to argue that Whewell was an inductivist and not a proponent of the hypothetico-deductive method (Buchdahl, 1971; Fisch, 1991; Hull, 2000; Ruse, 1975; Wettersten, 1992, 2005; Yeo, 1993) or of abduction (Achinstein, 1992; McMullin, 1992). Her arguments about the latter point are quite brief. What is missing is an analysis of the difference between inductive and abductive inference, which I will address in chapter six.

Mill addressed Whewell's philosophy of science through successive editions of his *System of Logic* (1843a, 1856, 1865b). Mill relied heavily on Whewell's (Whewell, 1837a, 1837b) *History of the Inductive Sciences* for both historical and contemporary information on scientific "generalities and processes" (Mill, 1874, p. 208), though Mill had some direct experience with botanical collecting (Curtis, 1988). Whewell's responses to Mill came chiefly in his (1849) *Of Induction, with Especial Reference to Mr. J Stuart Mill's System of Logic* and (1860) *Philosophy of Discovery* (the third part of the third edition of the *Philosophy of the Inductive Sciences*). Whewell had written his *History of the Inductive Sciences* (1837b) explicitly as a preliminary to his *Philosophy of the Inductive Sciences* (1840c). In addition to this historical work, Whewell drew on his own experience studying mineralogy (Whewell, 1828), tidal science, and (I shall argue) historical architecture (Whewell, 1830; Whewell & von Lassaulx, 1842).

The debate was lively in part because each philosopher considered the philosophy of science to be linked to societal reform, just as did early twentieth century philosophers of science (Douglas, 2009). In the last book of the *System of Logic* Mill attempted to apply his philosophy of science to

moral and political sciences. Mill began the book with Comte's (1830, pp. 47-48) claim that societal crisis could only be solved by the development of a universal philosophy (Mill, 1843b, p. 474), and cited Bacon as an example of pointing the way (Mill, 1843b, p. 477). Mill claimed that the methods of moral science must follow from the principles of science in general, which he took himself to have described in the *System*. Whewell signaled his own intention to emulate Bacon through the title of his *Novum Organon Renovatum* (1858). For both philosophers, the goal was to analyze principles of reasoning that could be applied universally, and the method was to extract these principles from the study of science. I begin with Mill's efforts.

## 2.0 MILL'S PHILOSOPHY OF SCIENCE

In this chapter, I introduce the core elements of Mill's philosophy of science. The first section analyzes what I will call Mill's network assumptions about the role of causal reasoning and laws in scientific theories. The assumptions will be critiqued by Whewell (chapter 3); and are at the core of twentieth century debates on explanation and the status of the historical sciences (chapter 4).

In addition to laying the groundwork for chapters to come, the present chapter clarifies debates about natural kinds. Debates about the ontology of species have run together distinct ideas about natural kinds, and I disentangle these ideas by reference to their historical roots in Mill. The confusion turns on the question of the relation between natural kinds and theories about causal processes, where causal process theories are understood in terms of Mill's framework. I argue that Mill held a view on natural kinds that is quite distinct from, and potentially at odds with, his views about causal induction. The problem stems from Mill's claims, on the one hand, that his account of induction of causes is an account of all scientific induction (Mill, 1843a, p. 435); and on the other that induction that identifies natural kinds is distinct from induction of causes (Mill, 1843b, p. 120).

## 2.1 MILL'S INDUCTION AND CAUSAL REASONING

### 2.1.1 Induction: Particulars and General Propositions

Book I of the *System of Logic* develops Mill's philosophy of science through his philosophy of language and classification. To Mill, full-fledged scientific induction centers around the formation of



a general proposition asserting a relation between phenomena. General propositions group past particular experiences by means of general names. Mill considered what he calls a mistaken view of how general names work:

“There is a kind of language very generally prevalent in these discussions, which seems to suppose that classification is an arrangement and grouping of definite and known individuals: that when names were imposed, mankind took into consideration all the individual objects in the universe, made them up into parcels or lists, and gave to the objects of each list a common name...” (Mill, 1843a, p. 126)

On the mistaken view, whether an object belongs in a given class can be determined by looking for the object amongst the listed (denoted) objects of the class. Mill noted that, when the mistaken view is explicitly described as he has done, no one would endorse it. He claimed that the view is implicit in logicians’ attempts to handle syllogistic reasoning in terms of classes which are understood to be defined extensionally. For example, Mill complained that “all men are mortal” is handled as a claim that the collection of all men is a subset of the collection of all mortal entities.

Against this view, Mill argued that a class is formed, logically, by a general name, whose only definite meaning derives from an indefinite grouping of individuals with definite attributes (Mill, 1843a, p. 127). The definite meaning of the general name of a group is its connotation: definite attributes that various particular objects may be found to possess. The grouping of individuals is “indefinite” in that we need not know how many objects the class denotes. Class membership may fluctuate, with individual objects coming and going.<sup>1</sup>

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<sup>1</sup>Indeed, the general name of a class can retain its meaningfulness even when the class does not contain any really existing objects. The group ‘dodoes’ continues to be a meaningful class even if, at some particular time, there are no dodos in the universe. Some meaningful groups are mathematical abstractions, such as “line” without breadth, that do not physically exist. Moreover, distinct general names can retain distinct meanings through connotation even when the objects denoted by the names are found to be the same. For example, Mill claimed that if we discovered that gold is the only metal, the name “metal” would retain a meaning distinct from the name “gold”.

Louis Agassiz posed his students a similar example to illustrate Agassiz’s view that taxonomic categories have meaning beyond the denoted set of organisms considered as individuals (Winsor,

Mill offered a zoological example to demonstrate reasoning about general propositions: the claim that all oxen ruminates (Mill, 1865a, p. 425). The speaker does not consult all objects found in the class designated “oxen” and find that each such object is also to be found in the class designated “ruminators”. Nothing is predicated of the class “ruminators”, for the speaker does not have the class “ruminators” in mind at all. Rather, according to Mill, the speaker asserts that whenever some particular entity may be found that meets the attributes connoted by “oxen”, that same particular entity will be found to ruminates (to bear the attribute, “ruminates”).

Class formation, for Mill, is an ongoing process that collects experiences and sorts them via attributes that flag meaningful resemblances between experiences. The “essential properties” of a thing just are the attributes connoted by the word that refers to the thing (Mill, 1843a, p. 148), so that the essential properties of “man” are those attributes by which the speaker refers to things that are called “men”.<sup>2</sup> As will be elaborated below, to Mill, scientific inquiry is ultimately a matter of

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1991, pp. 14-16). Agassiz argued that, even if the entire *embranchement* of Articulated animals (Arthropoda) contained only one species, the American lobster, still the lobster would have defining characteristics when considered at each rank in the taxonomic hierarchy (that is, the plans of the phylum Arthropoda, subphylum Crustacea, class Malacostraca, order Decapoda, genus *Homarus*, species *americanus*) (Agassiz, 1859, p. 5). The general name Arthropoda retains meaning distinct from the meaning associated with each sub-category and distinct from the collection of individual lobster objects.

<sup>2</sup>My reading of Mill here is consistent with Snyder (2006, p. 163), who claims that “Mill argued... that so-called essential propositions are merely verbal ones. That is, he claimed that objects have essential properties only insofar as the class is described by a connotative name which gives the properties as part of the connotation.” However, Snyder attributes Mill’s rejection of Aristotelian essences (essences in the sense of the “metaphysicians” and “schoolmen”) to a rejection of all necessity other than verbal. I will argue below (page 14), contra Snyder, that Mill does not reject metaphysical causal necessity.

Mill credited Locke with correcting the scholastics by equating “essential properties” with the signification of names. Mill labeled this Locke’s most needful and valuable contribution to philosophy (Mill, 1843a, p. 150), though he does (as Snyder notes) criticize Locke’s distinction of Real from Nominal essences as a holdover of scholastic essentialism.

Mill’s “real propositions”, which refer to objects in the world via essential properties (attributes connoted by general names), should not be confused with what he calls “merely verbal

linking attributes to attributes, where the linked attributes are connotations of general names that group phenomena.

Mill noted that conscious perceptions are caused by sense impressions, themselves caused by spatiotemporal particulars. Thus, Mill argued, our inferences about facts in the world are ultimately about particulars. General propositions summarize inferences that are, essentially, claims about links between particular instances.

“All inference is from particulars to particulars: General propositions are merely registers of such inferences already made, and short formulae for making more: The major premiss of a syllogism, consequently, is a formula of this description: and the conclusion is not an inference drawn *from* the formula, but an inference drawn *according to* the formula: the real logical antecedent, or premisses, being the particular facts from which the general proposition was collected by induction. Those facts, and the individual instances which supplied them, may have been forgotten; but a record remains, not indeed descriptive of the facts themselves, but showing how those cases may be distinguished respecting which the facts, when known, were considered to warrant a given inference.” (Mill, 1843a, p. 259).

Mill’s induction can proceed either from particulars to particulars, or from particulars to general propositions. Mill argued that very much of our reasoning takes the form of particular-to-particular, without the formation of a general proposition (Mill, 1843a, pp. 251-258). As a matter of usage, Mill typically reserved the term “induction” for cases in which the reasoner forms a general proposition (Mill, 1843a, p. 274). The general proposition can be pragmatically useful, helping us remember and communicate to others what past experiences we have had. The act of formulating the general proposition can serve as a check of our reasoning, by calling to mind cases that may serve as counter-examples (Mill, 1843a, p. 265).

Mill claimed that the general proposition adds no logical force to our reasoning (1843a, p. 280). The past particulars, in themselves, are what confer logical necessity (to the degree that our

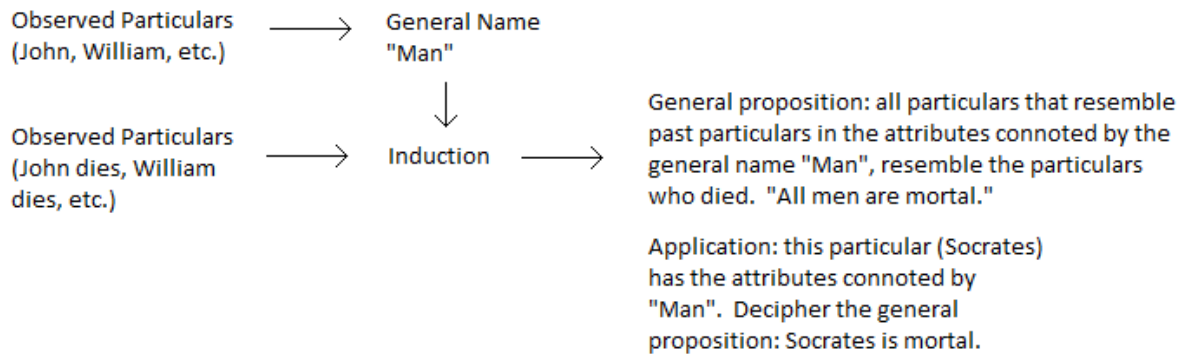
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propositions” (Mill, 1843a book 1, chapter 6) that are propositions about the signification of words (for example the assertion, “*Man* means *rational animal*”).

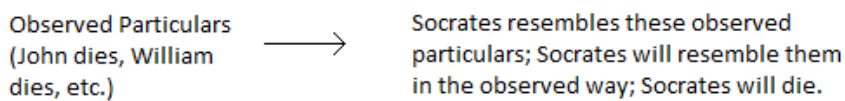
reasoning is sound, and to the degree that there is any certainty to be had) to conclusions drawn from general propositions. The force of any inference comes solely from past particulars, and general propositions “are but collections of particulars, definite in kind but indefinite in number” (Mill, 1843a, p. 347). Particulars are “definite in kind” in that they are truly subject to the general proposition – they meet the criteria that the general proposition was formulated to express. Yet we should not think of general propositions as merely extensionally defined sets of a finite number of past particulars; the particulars that a general proposition refers to are “indefinite in number”. The general proposition asserts a claim about a general name. This is a claim about any particulars that bear the attributes that the general name connotes. The speaker need not have any specific individual(s) in mind. The speaker refers to classes of entities as types that summarize token particulars.

Mill illustrated the relationship between inductive and deductive reasoning using the general proposition “all men are mortal” and the conclusion that “Socrates is mortal”. The work of induction substantially consisted in forming the general proposition (1843a, p. 263). Deduction, according to Mill, is the subsequent recognition that the general proposition applies to Socrates. The assertion that Socrates is a man amounts to the claim that Socrates resembles previously observed individual men in the ways that are relevant to the formation of the general name, “men” (1843a, p. 272). Once this resemblance is established, application of the general proposition is simply “deciphering our own notes” without any further ratiocination. Deduction substantially consists in interpreting the general proposition with respect to a proposed case (1843a, p. 274). The force of the reasoning that supports the conclusion, Socrates is mortal, is the collection of observed cases in which men died together with the evidence that Socrates relevantly resembles these men. To Mill, the logical force supporting deduction is ultimately the same force that drives induction.

### Induction: particular to general proposition to particular



### Direct particular to particular reasoning



Mill claimed that even geometrical examples fit his view of induction, illustrating with the axiom that all radii of a circle are equal (1843a, p. 256). A typical geometric proof proceeds to diagram some particular circle, ABC, and prove that all radii of ABC are equal. This is a particular statement about the one particular circle, ABC:

“One instance only is demonstrated: but the process by which this is done, is a process which, when we consider its nature, we perceive might be exactly copied in an indefinite number of other instances; in every instance which confirms to certain conditions.” (Mill, 1843a, p. 257).

We then form a general proposition in order to assert the particular fact plus the possibility of extending the particular truth to other instances, definite in kind (that is, meeting the conditions that guided us in diagramming the circle ABC, namely the attributes of “circle”) but indefinite in number.

“The contrivance of general language furnishing us with terms which connote these conditions, we are able to assert this indefinite multitude of truths in a single expression, and this expression is the general theorem.” (Mill, 1843a, p. 257).

In Mill’s view, induction is a summary of links between particular resembling experiences.

We have had past experiences in which the antecedents obtain; in those experiences, the

consequence obtained. The general proposition may also indicate that in past cases we have reasoned from the antecedent to an expectation of the consequence, and that we consider these inferences to have been warranted. Finally, the general proposition asserts that such inferences will be warranted in future cases: we expect that in future instances that resemble these past cases in the relevant ways, when the antecedents obtain the consequence will obtain.

### 2.1.2 Justifying Induction: Causal Necessity

The challenge for Mill's account of induction is the same challenge that faces accounts of induction in general: to justify the inference from past particulars to the conclusion about indefinitely many future observations. The logical force of induction derives from some kind of statistical consideration of a set of past experiences. C.J. Ducasse (1924, p. 23) argued that Mill's account of induction misses precisely the aspect of induction which can address the challenge of justifying induction. On Ducasse's view, the inductive conclusion derives force not just from the fact of having a stock of past experiences of the relation that is expressed by the general proposition. The sampling procedure used in drawing from experience is crucial. Ducasse pointed out that to form a valid inductive inference, we need a sample of past experiences that is both large and selected at random.

“Thus the inference is not from particulars *as particulars*, but from particulars *as picked in a peculiar manner*, which manner *renders them jointly equivalent to a universal*. Inference from particulars merely as such, to particulars, is logically worthless, that is, it is not really inference at all.” (Ducasse, 1924, p. 91).

The challenge for Ducasse then is to give an account of how to render particulars equivalent in logical force to a universal.

Mill took another tack, attempting to ground the logical force of induction in an appeal to causation. As will be argued below (section 2.2.1), this strategy omits some cases of induction – induction to “uniformities of coexistence” about the identity of natural kinds. Section 2.2.3 addresses attempts to justify this other type of induction. Mill sometimes intended only causal induction, the subject of the present section, when he (carelessly) used the general term “induction”. Hence Mill’s description of the theory of induction as “essentially an inquiry into cases of causation” (Mill, 1843a, p. 435).

Successful instances of causal induction pick out the links between past experiences that are causal links between particular phenomena. The epistemological challenge then becomes, how do we identify those cases in which a causal connection obtains? Mill’s methods of causal inquiry attempt to meet this challenge (Mill, 1843a, p. 450). Whewell’s objection to Mill’s methods will be considered in chapter 3, and will recur as I untangle subsequent history of philosophy of science in relation to the two philosophers’ contrasting ontologies (chapter 4).

Science aims to formulate laws that assert cases of causation, via general propositions that function as described above. Definite attributes group classes of entities that we figure as causes and effects in particular observed causal transactions. The general proposition asserts: all those things that resemble these previously observed entities, in ways relevant to the formation of the proposition (that is, in the *attributes* connoted by the general name), will resemble those entities as well in that the causal transaction will occur just as it did in those observed cases. General propositions that express such reliable cases of causation are Millian causal laws. Mill’s causal ontology is a network of transactions between particular entities; Mill’s science is a set of generalized claims about these causal transactions.

The view of causation as a “leash” between atomic events goes back to Hume, and Mill found himself faced with Hume’s problem of justifying the assumption of a necessary connection

that joins cause and effect. Snyder (2006) has argued that Mill adopted what is generally called the Humean view of causation in response to this problem. On the Humean view, causation is mere regularity, observed constant conjunction without any real necessary connection between prior cause and succeeding effect. If Snyder's interpretation were correct, Mill's account of scientific inquiry as I have sketched it above would be problematic because it appeals to causal necessity to justify induction. Here I argue that Mill had a more robust view of causation, and indeed held that some form of real causal "force" underwrites the causal uniformity assumption.

Mill's occasional statements of skepticism about the necessary connection appear to support Snyder's case. Mill stated that when he speaks of scientific investigation of causation, he has "nothing in view" other than "constant relations of succession" (Mill, 1843a, p. 422). Mill expressed support for Comte's claims that we cannot investigate the nature of causes themselves (Comte, 1858, p. 28; Mill, 1891, p. 6). We can only investigate causal laws in the form of constant relations of unconditional succession (Mill, 1843a, pp. 422-423).

However, the target of this skepticism is not the reality of causation itself, but our epistemic access to the necessary connection between cause and effect. The skeptical statements reflect Mill's view that we have no direct epistemic access to the causal necessity in itself, a view descended from Hume. Because all reasoning, for Mill, is an extension of individual experiences, reasoning about causal connection is the extension of experiences of constant regularity between particular events and entities. From an epistemic point of view, any reference to causal connection is a reference to the set of these past occurrences together with the expectation that the regularity will obtain in future experiences. There is no direct reference to any causal necessity linking the occurrences.

In *An Examination of Sir William Hamilton's Philosophy* (1865a, pp. 306-307), Mill traced the origin of our idea of causal necessity to our experiences of the world. According to Mill, the idea of causal necessity derives from the feeling of *effort* that accompanies our voluntary movements. We



extend this feeling to external phenomena, supposing that there must be an analogous causal “power” between all causal antecedents and consequents.<sup>3</sup>

Hume had offered a similar psychological account of the origin of the idea of causal necessity. Hume argued that there is no reason to expect the psychological account to line up to any true causal necessity in the world, and (on the standard Humean account) accepted the skeptical conclusion that there is no necessary connection between cause and effect. Mill, however, did not reason that the availability of the psychological account should lead to skepticism. Our idea of the necessary connection, however derived, might indeed line up with a really existing causal necessity in the world. The lack of satisfactory justification for our epistemic access to the necessary connection between cause and effect should not drive us to reject the existence of the necessary connection in nature.

The way in which Mill developed his philosophy of science presumes the reality of the necessary connection. Hume (1888, p. 173) famously and problematically followed up his skeptical conclusion with a set of rules for differentiating real cases of causation from mere observed regularities. Given that Mill does not directly express the Humean skeptical conclusion, it is less problematic that Mill also recommends methods for discerning real cases of causation (Mill, 1843a, p. 437 and onward; Mill discusses the four or five methods of experiment from p. 450). Simple, uncritical observation can ascertain “uniformity of succession”, but cannot prove causation (Mill, 1843a, p. 448). The critical experimenter must reproduce the effect in various circumstances in order

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<sup>3</sup>Mill may have been influenced here through his reading of Herschel (1831, p. 149). Herschel attributed causal reasoning in some cases to analogizing direct experience of “force”. However, in Herschel’s view (p. 150), we reason about causal necessity via analogy to force in only some instances of causal reasoning (including the simplest ones). Mill argued that the psychological idea of causal necessity itself is derived from the idea of effort. Mill’s dew example 490-519 comes from this same passage of Herschel (1831, pp. 159-163). The passage also includes Herschel’s versions of methods of experimental inquiry.

to know that the uniformity is unconditional. One might try to reduce Millian causation to mere regularity, with experimentation necessary to establish that the regularity is uniform and invariable. But if simple observation without experimentation can ascertain uniformity, it seems that what the experimenter is doing is testing the causal necessity.

Ducasse (1924, p. 11) made a similar argument specifically regarding Hume's fourth rule "by which to judge of causes and effects". The rule states:

"The same cause always produces the same effect, and the same effect never arises but from the same cause. This principle we derive from experience, and is the source of most of our philosophical reasonings. For when by any clear experiment we have discovered the causes of effects of any phenomenon, we immediately extend our observation to every phenomenon of the same kind, without waiting for that constant repetition, from which the first idea of this relation is derived." (Hume, 1888, p. 173).

Ducasse pointed out that, if causation were simple invariable succession, Hume's fourth rule is not a method to judge that a causal relation obtains but rather the definition of causation. Yet the principle is supposedly derived from experience separately from our idea of causation itself. Hume's qualification that regularity is the source of our "first idea" of the causal relation suggests that the relation involves some additional element(s). On these grounds, Ducasse argued that Hume implicitly uses the word "cause" in a sense other than invariable regularity (Ducasse, 1924, p. 12).

Similarly, the content of Mill's methods for investigating proposed causal relations suggest that the experimenter is seeking to establish causal necessity beyond mere regularity. For example, Mill described an experiment to prove a causal connection:

"Suppose that, by a comparison of cases of the effect, we have found an antecedent which appears to be, and perhaps is, invariably connected with it: we have not yet proved that antecedent to be the cause, until we have reversed the process, and produced the effect by means of that antecedent. If we can produce the antecedent artificially, and if, when we do so, the effect follows, the induction is complete; that antecedent is the cause of that consequent. But we then have added the evidence of experiment to that of simple observation. Until we had done so, we had only proved *invariable* antecedence but not *unconditional* antecedence, or causation." (Mill, 1843a, pp. 447-448)

It is striking that Mill described the induction as complete without requiring repetition or variation of the experimental setup. It appears that testing whether the connection is unconditional does not mean testing whether the connection holds on distinct occasions and under various circumstances, that is, invariably. The ability to produce the effect provides a distinct type of test than ascertaining invariability.

The “unconditional” component of Mill’s description of causation is some sort of causal necessity, some causal “force” beyond mere constant succession. This point underlies Joseph’s (1916, p. 113) criticism of Mill’s definition of cause “as the invariable and unconditional antecedent of a phenomenon”, on the grounds that “unconditional” cannot be explained unless the concept of “cause” is already understood. Causation asserts more than that the effect always has followed, but that the effect always must follow: else, as Reid pointed out, the day would cause the night (Mill, 1843a, p. 409).

In an example that illustrates negative causation, Mill distinguished positive causes as “producing” causes; negative causes are not producing, but simply the absence of preventing causes. The causal necessity, whatever it is, lies behind producing causes, while negative causes are just absences of things that would have been producing (that is, if the negative cause were present, it would prevent the observed outcome by producing a different one). The sum total “cause, philosophically speaking” can be calculated as a vector addition of causes, so that both positive and negative causes are accounted for in descriptions of causal transactions.

“The cause, then, philosophically speaking, is the sum total of the conditions, positive and negative, taken together... the negatives are summed up as, the absence of any preventing causes.” (Mill, 1843a, pp. 404-405).

Despite parity in calculation, however, Mill retained an ontological distinction between absent, potential causes and positive causes that actually exert themselves.<sup>4</sup> This exertion, whatever it is, is the force of causal necessity, the source of unconditionality. Snyder (2006) claimed that for Mill, “unconditionality” does not involve necessity but instead asserts that there is no condition external to the cause that contributes to the effect. Given that Mill defines “the cause” as including any and all conditions, it is difficult to see what could be added by the claim that a cause must be “unconditional” in this sense.

Some of Mill’s skeptical language about causation appears in the context of calculating contributions of distinct causes. This calculation is performed in terms of constant conjunction, making no assertions about the nature of operative causes. Though the vector addition of causal factors assumes causal necessity, the calculation does not address the causal necessity directly. All that is needed is a grasp of particular constant relations, and evidence that the relation described by each causal law is “unconditional” – i.e. that the causal necessity obtains in the proposed relation.

Mill cited Thomas Reid in distinguishing unconditional causation from mere regularity (Mill, 1843a, p. 409); in Reid we find language that perhaps inspired Mill’s skeptical statements:

“Natural philosophers, who think accurately, have a precise meaning to the terms they use in the science; and when they pretend to show the cause of any phenomenon of nature, they mean by the cause, a law of nature of which that phenomenon is a necessary consequence.” (Reid, 1843, p. 106)

The phenomenon is a necessary consequence of the law in the sense of logical entailment. Some further element is required but unobservable:

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<sup>4</sup>Stegmann (2012) explicated “Milleian parity” as the view that there is no ontological distinction between conditions and causes. Stegmann did not discuss negative causes. Milleian parity must be understood with the distinction between positive and negative causes in mind. When adding partial causes, Mill proposes a “convenient modification of the meaning of the word cause, which confines it to the assemblage of positive conditions without the negative” (Mill, 1843a, p. 340).

“But supposing that all the phenomena that fall within the reach of our senses were accounted for from general laws of nature, justly deduced from experience; that is, supposing natural philosophy brought to its utmost perfection, it does not discover the efficient cause of any one phenomenon in nature.

“Upon the theatre of nature we see innumerable effects, which require an agent, endowed with active power; but the agent is behind the scene.” (Reid, 1843, p. 107).<sup>5</sup>

Reid’s claim that we can never discover causal necessity goes hand in hand with his view that the necessity is real:

“The laws of nature are the rules according to which the effects are produced; but there must be a cause which operates according to these rules. The rules of navigation never navigated a ship. The rules of architecture never built a house.” (Reid, 1843, p. 107).

As is the case with Reid, Mill’s skepticism about our epistemic access to causal necessity is compatible with the view that causal necessity is real in the world. The skepticism expressed here is about our epistemic access to the nature of the causal necessity in itself.

Reid’s examples, however, seem to locate the force of causation in entities: the ship’s pilot, the architect and builders. In her account of causation as rooted in entities with capacities, Nancy Cartwright (1994) cited Mill as a proponent of object-causation. Schmidt-Petri (2008) has convincingly countered Cartwright’s historical claim. Mill held that causation is fundamentally a relation between two phenomena, describable by causal laws that express regularities in which necessity obtains. The view that causation is fundamentally a link between entities perhaps renders the force of causal necessity more mysterious than locating necessity in capacities of identified entities. The relational link is not directly accessible; Reid’s move is to locate the causal necessity in the entities themselves. Mill leaves the necessity in the link.

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<sup>5</sup>The passage continues: “It is only in human actions, that may be imputed for praise or blame, that it is necessary for us to know who is the agent; and in this, nature has given us all the light that is necessary for our conduct.” This may be another source for Mill’s claim that our idea of the causal power derives from the experience of voluntary action (footnote 3).

Mill was by his own testimony influenced by Auguste Comte, but Mill described Comte's outright rejection of the word "cause" as a mistake:

"He fails to perceive the real distinction between the laws of succession and coexistence which thinkers of a different school [i.e. Whewell] call Laws of Phenomena, and those of what they call the action of Causes: the former exemplified by the succession of day and night, the latter by the earth's rotation which produces it." (Mill, 1891, pp. 57-59).

Mill claimed that this error prevented Comte from conceiving an inductive logic, which could only be based upon the law of universal causation, that every phenomenon "has some phenomenon other than itself, or some combination of phenomena, on which it is invariably and unconditionally consequent" (Mill, 1891, pp. 58-59). To Mill, induction requires the universal causation assumption (Mill, 1843b, pp. 107, 126; 1865a, p. 400).

In several places, Mill claims the universal causation assumption is itself arrived at and grounded on induction (Mill, 1843a, p. 372; 1843b, p. 112; 1865a, p. 537). Mill dealt with the obvious circularity problem rather unconvincingly. He claimed that this is a case of induction by simple enumeration, not "complete" Induction (which requires the universal causation assumption), so that strictly speaking the law of universal causation is itself only an empirical law. Empirical laws describe observed regularities without making claims of causal necessity. Yet Mill claimed the law of universal causation is so strongly supported by all human experience that the distinction between mere observed regularities and laws of nature breaks down in this case (Mill, 1843b, pp. 112-113).

If Mill's argument that causal necessity is grounded by induction is viciously circular, he might simply take the law of universal causation on board as an assumption (one that all humans do and must make; though Mill admits that this fact does not prove that the law is true - Mill, 1843b, p. 109). In any case Mill's account of induction requires causal necessity.

### 2.1.3 Justifying Induction: Relevant Resemblances

Mill's causal induction requires one to discern when particular experiences are united by resemblances relevant to the formation of causal laws:

“It [induction] consists in inferring from individual instances in which a phenomenon is observed to occur, that it occurs in all instances of a certain class; namely, in all which *resemble* the former, in what are regarded as the material circumstances” (Mill, 1843a, p. 370).

The “material circumstances” are the resemblances that reliably mark that the causal transaction will occur as our causal law asserts. Scientific theories include causal laws that refer to entities via general names. The general names connote attributes that are the relevant resemblances between experiences. The challenge here is to identify the properties by which a scientific theory identifies causal relations. How do we choose resemblances to serve as attributes in a general name, such that the general name reliably picks out entities that function as causes or effects?

Mill's answer is that we should look to past experiences. We have experience of what kinds of uniformities tend to persist, what kinds of regularities we can rely on. Experience cannot inform us about the nature of causal necessity, but it can indicate what kinds of cases and resemblances are underwritten by causal necessity. The relevant experiences are collected into causal laws that indicate the kinds of entities that figure in causal transactions. Mill's methods of experiment were intended to guide experience and provide more precise knowledge about which resemblances we can rely on in identifying these kinds.

Mill claimed that the appeal to scientific background knowledge distinguishes induction from what he calls the “method of analogy” (Mill, 1843b, p. 98). To illustrate, Mill posed the question, is there life on the moon? Proceeding by the method of analogy, we might investigate properties of the moon and of the earth (where life is known to occur). The moon resembles the earth in being opaque, nearly spherical, subject to gravity, at a similar distance to the sun, etc. If these were the only

properties we investigated, these resemblances would support the claim that the moon will resemble the earth in the property, bearing life (Mill, 1843b, p. 100).

A significant problem with what Mill calls the method of analogies is the lack of guidance as to which properties should be investigated. The method of induction improves on the method of analogies precisely in consideration of properties' causal relevance. Physiological science provides the experience that tells us what kind of properties are more or less relevant to the question at hand. Experience tells us that organisms breathe: removed from air, they die. Experience tells us that organisms require water: when completely dehydrated, they die. The moon lacks atmosphere and water, and therefore we can expect that there will not be life on the moon.<sup>6</sup> The conclusion here is better supported than that reached by simple analogy from indifferent resemblances, because we focus on the attributes connected with "life" in causal laws formed by induction. The causal relevance of these properties was established by experience, namely the empirical investigations of physiological science.

For Mill, certain resemblances are hypothesized to be reliably linked to the causal structure that underwrites successful induction. Mill directed us to consult past experience in determining "the material circumstances" that unite experiences as a type that figures in a causal law (Mill, 1843a, p. 370). In particular, we should turn to the experiences codified by an empirical science. A complication arises in the case that distinct scientific research programs draw on different experiences, and/or offer different conceptualizations of potentially relevant properties.

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<sup>6</sup>Mill inferred these facts from the claims that if the moon had an atmosphere, the atmosphere would refract light, and if the moon had water it would have clouds. We observe that the moon lacks clouds and atmospheric light refraction.



#### 2.1.4 Mill's Causal Ontology

My chapter thus far has concerned classification of the phenomena of experience, and the problem of identifying causally relevant resemblances between experiences. Mill's concern in this type of classification is to identify properties that reliably mark causal laws, which are the primary targets of scientific inquiry. Thus the problems of classification are central problems for Mill's inductive science in general. There is some fuzziness in causal ontology here. Mill sometimes specified that causal links obtain between events:

“It is *events*, that is to say, *changes*, not substances, that are subject to the law of Causation.” (Mill, 1865a, p. 295).

However, as Ducasse (1924) pointed out, what is most fundamental to Mill's view is that causation is a relation between entities – the relata of causal laws that are the subject of scientific inquiry. Indeed Ducasse argued that Mill's view is mistaken because Mill fails to specify that causation obtains between “*single, individual events*” (Ducasse, 1924, p. 19). Mill's concern in the above quote is to counter William Hamilton's claim that the doctrine of universal causation is equivalent to the belief that nothing begins to exist, because every effect must somehow already be present in the cause. Mill's claim that substances are not the subjects of causal laws appears to contradict statements he made elsewhere. He wrote of events, facts, states, objects, and properties as entering into cases of the causal relation (see Joseph, 1916, pp. 404-405 for discussion; Mill, 1843a, pp. 72, 402, 437). In all of these cases, the entities of particular causal transactions are understood as tokens of types that are cited in causal laws.

On Mill's view, science aims at laws that describe causal transactions between token entities. This view would be tremendously influential in subsequent philosophy of science. I trace this influence and associated problems in chapter 4, using Whewell's philosophy of science (chapter 3) as

a foil. First, however, I draw some lessons from the relation between Mill's philosophy of science and his view of natural kinds, to which I now turn.

## 2.2. NATURAL KINDS

### 2.2.1 Mill on Natural Kinds

John Wilkins (2012) has argued that Mill was the first philosopher to collapse the historical distinction between logical species and biological species (though Wilkins noted that Mill sometimes does make the distinction - e.g. Mill, 1843a, p. 169). Wilkins' broader concern was to counter the claim that pre-Darwinian systematists conceived biological species as natural kinds, defined in essentialist terms. However, as I will shortly argue, Mill did not hold the views about natural kinds that Wilkins ascribed to him. Nonetheless my arguments support Wilkins' broader claim about pre-Darwinian systematists by urging a richer picture of historical views on the relationship between species, natural kinds, and scientific laws. The philosophical lesson is that several ideas need to be separated: the idea that science identifies types as relata of causal laws; the idea that natural kinds (perhaps including biological species) are these types; and the idea that natural kinds are defined by possession of a core group of necessary and sufficient properties.

Twentieth and twenty-first century philosophy of science inherited Mill's concern for classification of the entities studied by inductive sciences. Philosophers of biology, in particular, have discussed natural kinds as basic entities of scientific laws and scientific theories (e.g. Ghiselin, 2005, who traces his account of natural kinds to Quine; Quine, 1974, 1979). David Hull explicated the connection as follows:

“Any kind term that appears in a law of nature is a genuine natural kind. Any putative kind term that does not is suspect. Of course, this criterion merely shifts the problem to distinguishing laws of nature from other sorts of generalizations. Once again, none of the suggested criteria work all that well, but the one I favor is figuring in a scientific theory. A putative law of nature that remains in inferential isolation from all other putative laws is suspect.” (Hull, 1983, p. 184).<sup>7</sup>

When combined with Mill’s account of laws in science (2.1), this account of natural kinds identifies natural kinds with the relata of causal transactions. On this view, the search to identify natural kinds is the search to identify the entities that are linked by causal transactions.

The view is often tied to claims that natural kinds are identified via definite properties that are relevant to causal transactions. Causal laws are supposed to hold irrespective of spatiotemporal location: they describe regularities that hold throughout all time and space with respect to the specified entities. This generalizability implies that the identified subjects of causal laws are “immutable”. While members of kinds change, the defining properties by virtue of which entities participate in causal laws must remain the same. If *gold* is a natural kind, any individual sample of gold must bear the properties by virtue of which gold participates in the causal transactions described by causal laws. These properties do not change across space or time. Any entity that bears these properties, regardless of its spatiotemporal location, must be gold. On this picture, we can conclude that natural kinds must have some core properties, possession of which is necessary and

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<sup>7</sup>Following the quoted passage, Hull cited Whewell’s consilience of inductions as supporting Hull’s view. He then distinguished two separate senses of consilience, noting that only the first applies to natural kinds. This first sense is the explanatory extension of a scientific generalization across domains. The second sense addresses the spatial decomposition of complex systems. When distinct principles of decomposition agree about the spatiotemporal boundaries of the system, this consilience indicates that the target system is a natural individual.

The view that I develop in subsequent chapters is that explanatory extension across domains is a key criterion for systematic inference. However, the relevant type of explanation need not cite causal laws (chapter 5). Consequently, the first consilience criterion need not support the idea that natural kinds are theoretical entities that are the subjects of causal laws.

sufficient for an entity to belong to the natural kind. It is sometimes possible to identify the core properties on which kind membership depends and which are responsible for observable resemblances among kind members. A paradigmatic example is gold: all individual pieces of gold share resemblances that are caused by shared possession of the atomic number 79. Observable resemblances include ductility, color, density, and behavior when subjected to specific chemical reactions.

Wiley and Lieberman summarized these views:

“Natural kinds, as kinds associated with general theories about processes that occur in the real world, are eternal and immutable, unbounded by either time or space... For example, the natural kind ‘helium’ falls out of, or is integral to, theories of atomic physics as the natural kind of atom that has two protons. Theories of atomic physics explain why particular atoms have two protons, how having two protons confers properties such as chemical reactivity (or lack thereof), and under what circumstances one expects individual helium atoms to originate in nature.” (Wiley & Lieberman, 2011, p. 25)

Wilkins ascribed the above set of views about natural kinds to Mill and argued that a major historical shift occurred here: “Mill’s disagreement with Whewell marks a tidal change from taking taxonomy in natural history seriously to philosophy attempting to define a priori what a scientific class must be” (Wilkins, 2012, pp. 9-10). Wilkins’ historical thesis was that biology took the Millian “tine” of a “taxonomic fork”, until the advent of phylogenetics restored a more Whewellian approach to classification.

“Mill’s tine of the fork supposes that we merely establish terms around types, and that when we have a proper understanding of the kind, we are able to give a full account of all and only those properties that cause the types denoted by the terms to come into focus (and which may force a revision of the terms...”

“Mill supposes that if there is a natural kind... then that natural kind must have necessary causal properties that makes it what it is.” (Wilkins, 2012, pp. 11-12).

Some philosophers have argued for alternate conceptions of natural kinds. Alternative accounts break from the above-outlined reasoning in a variety of ways. Natural kinds may be united by a cluster of properties, so that the possession of any particular individual property is neither

necessary nor sufficient for inclusion in the kind (Boyd, 1991). This idea modifies the above picture by providing an alternative principle for the grouping of natural kinds as causally relevant types; an alternative reason that the natural kind functions in causal laws.

Another alternative, proposed by Paul Griffiths (1999), is that natural kinds may be united by historical essences so that inclusion in the kind is not, in principle, independent of spatiotemporal location. Griffiths' view appears to be at odds with the view that natural kinds are the subjects of causal laws that hold across all time and space. Viewing natural kinds as historical entities does not seem to fit with Mill's philosophy of induction, on the assumption that natural kinds are the subjects of causal laws. One way to reconcile the views would be to revise accounts of causal laws, to introduce some other account of causal theorizing about spatiotemporally restricted causes (such as Whewell's *historical causation* – see chapter 5). Another approach is to separate the concept of natural kinds from the task of formulating causal laws of Milleian form. In fact, there is precedent in Mill's own work for such a separation.

There are places where Mill seems to equate the identification of core causal properties with natural classification:

“The properties, therefore, according to which objects are classified, should, if possible, be those which are causes of many other properties; or at any rate which are sure marks of them.” (Mill, 1843b, p. 302).

Here and elsewhere (Mill, 1843b, p. 242) Mill included the proviso about “sure marks” and expressed doubt that we can identify core causal properties in practice (Mill, 1843a, p. 155). These doubts are consistent with a view that scientists seek reliable diagnostic properties as proxies for the true core properties, just as John Locke is typically described as describing nominal essences as a stand-in for identification of real essences.

However, in other places, Mill clearly identified a concept of natural kinds that are not united by joint possession of core causal properties. Some of Mill's nineteenth century commentators were

well aware of this point, which has largely been missed by recent commentators (but see P. D. Magnus, 2014, forthcoming). Read (1877) suggested that we can reduce all natural kinds to the effects of causation, but his suggestion was intended to *revise* Mill's view. Mill expressly denied that causal necessity suffices to bound natural kinds. Rather, natural kinds are united by uniformities of co-existence that are not dependent upon causation (Mill, 1843b, p. 120).

Mill introduced the natural kind concept through a historical account of the origin of group names. There is a strength in this approach. Other approaches to the relationship between natural kinds and scientific theories sometimes do not fit well with the history of specific kind examples. For example, Wiley and Lieberman (2011) linked natural kinds to scientific theories using the core property approach described above:

“... we can think of kinds as concepts associated with defining properties such that individuals (particulars) are either members of one kind or another. These defining properties are both necessary and sufficient. If a particular kind ‘falls out’ of a scientific theory (Quine, 1969) because the theory posits that certain entities should have certain properties if the theory is true, then it is termed a *natural kind* and the properties are said to be predicted by the theory. ... if evolutionary theory, broadly conceived, posits the existence of species in general, then this suggests that there is at least one natural kind, ‘species,’ that has properties that are manifested by particular species as part of their ‘speciesness’. Never mind that we do not fully understand the true nature and properties of all species (or all atoms or all planets), but it would be difficult to see how we could find a single particular species without some notion of what it is to be a species that is gained from theories about the world.” (Wiley & Lieberman, 2011, pp. 24-25)

On this view, what it is to be a member of the natural kind is to possess the core essential properties that define the kind as a type. Yet historically, recognition of paradigmatic natural kinds has preceded recognition of core causal properties. The development of the concept of elements was a significant scientific achievement that required grouping elements in terms of observed regularities and (ultimately) an understanding of the principles underlying these empirical groupings. Recognition of elements as basic kinds preceded recognition of the core causal properties responsible for the successful empirical groupings. Gold was considered a kind not only before the

identification of gold's atomic configuration, but before there was agreement that a core of micro-properties would explain the observable properties of gold samples. It is indeed difficult to see how one would have an idea of what it is to be gold, in terms of core causal properties, in the absence of a theory about the core atomic properties of elements in general. Mill provided an approach to natural kinds that did not require identification of core properties, nor the claim that core properties must exist.

In his account of general names, Mill claimed that usually when we form a general name, we do so with particular attributes in mind, "because we need a word by means of which to predicate the attributes which it connotes" (Mill, 1843a, p. 160). But in some cases, we recognize a need to group entities without explicit reference to any particular resemblances. Mill conceived of natural kinds as groups of objects that resemble each other in very many ways; in fact, the objects share innumerable many attributes in common. Precisely because they share so many attributes, we recognize quite easily that some objects belong together and group them without paying attention to any specific subset of attributes.

On Mill's view, this is not a provisional epistemic gap about the identity of core properties. In the case of true natural kinds, there is no core group of causally basic properties. Mill held that the shared attributes of a natural kind cannot be reduced to some few attributes that cause all the other resemblances. White things do not form a natural kind, even though they share innumerable attributes, in a sense: the attribute that Englishmen call them white, that Frenchman call them blanc, that they reflect more sunlight than do black things, etc. But all of these properties can be reduced to effects derived from the property of whiteness itself.

The *definition* of a natural kind cannot include all the attributes shared by objects forming the class because there are innumerable shared attributes. Scientific definition of natural kinds aims to specify boundary conditions: marks by which we can distinguish one kind from another (Mill,

1865b, p. 158). These diagnostic characters do not fully define kinds, because they do not specify all the attributes shared by the kinds. Our reference to the kind – the connotation of the name of the kind – consists of some diagnostic subset of attributes, together with the conviction that the kind is natural. This conviction is a claim that the kind members share innumerable many resemblances in common that are not reducible to consequences of other properties.

In keeping with Mill's philosophy of language, our idea of the kind-class is based on discrete groupings of shared attributes in general, rather than recognition of a finite group of concrete objects to be bounded. The name "human" is given to indicate a natural kind that shares innumerable many properties in common, and scientific investigation continues as to the nature of those properties. The name is not given in order to circumscribe particular identified individuals (John, William, etc.). Mill took it to follow that membership in the class may perpetually fluctuate.

Snyder (2006) argued that Mill did not think there are real natural kinds in nature. Snyder claimed that Mill rejected natural kinds because Mill rejected a real necessity in causation:

"Mill argued... that so-called essential propositions are merely verbal ones. That is, he claimed that objects have essential properties only insofar as the class is described by a connotative name which gives the properties as part of the connotation. ...

"Mill denied that individuals can be grouped according to the underlying traits or structure causally responsible for the production of their shared observed properties. This rejection of Lockean essences is based on his rejection of real causes other than invariable antecedents." (Snyder, 2006, pp. 163-164)

I have disputed Snyder's arguments about Mill's rejection of real causes above (section 2.1.2).

Though I disagree with her account of the reason for Mill's views on natural kinds, Snyder is correct to describe Mill's position as follows:

"Mill believed that scientists group things based only on conjunctions of observed properties that reflect no underlying causal structure." (Snyder, 2006, p. 28)

Mill indeed rejected natural kinds in the sense of groups linked by causally dependent properties.



However, Mill accepted the reality of natural kinds understood in a different sense. Mill evidently held that the existence of natural kinds, whose members share innumerable many resemblances in common, is simply a brute fact about the universe. Thus while Mill denied that the natural kinds are united by core properties responsible for their observed resemblances, he nonetheless held that natural kinds exist.

Snyder attributed confusion about Mill's position on natural kinds to "his own rather misleading statements" (Snyder, 2006, p. 162) affirming the existence of natural kinds (Mill, 1843a, pp. xi, 171-175; 1874, p. 221). In his autobiography Mill explained that by "working out the logical theory of those laws of nature which are not laws of Causation, nor corollaries from such laws, I was led to recognise kinds as realities in nature" (Mill, 1874, p. 221). What makes Mill's statements misleading is the tendency to run together distinct things that might be meant by "natural kind". In his account of induction Mill affirmed the idea of grouping together tokens of the relata of causal interactions to form causal laws about types. However, he held that there are really existing natural kinds that are wholly separate from this induction to causal laws. Mill held that there are natural kinds separated by joints in nature that are not the result of causes. He held that these kinds could not be defined by joint possession of core properties on which other properties causally depend. Rather, a Millian kind is defined by shared possession of innumerable properties that resist causal reduction.

### 2.2.2 Scientific Investigation: Biological Natural Kinds

Mill used some biological taxa as examples of natural kinds (Mill, 1843a, p. 171; 1843b, p. 313). He retained the "schoolmen's" definition of "species", as the "proximate (or lowest) Kind to which any individual is referrible [sic]" (Mill, 1843a, p. 168)" and claimed that humans constitute a species:

“There are indeed numerous sub-classes included in the class man... as for example, Christian, and Englishman, and Mathematician. But these, though distinct classes, are not, in our sense of the term, distinct Kinds<sup>8</sup> of men. A Christian, for example, differs from other human beings; but he differs only in the attribute which the word expresses, namely, belief in Christianity, and whatever else that implies, either as involved in the fact itself, or connected with it through some law of cause and effect. We should never think of inquiring what properties, unconnected with Christianity, are common to all Christians and peculiar to them; while in regard to Men, physiologists are perpetually carrying on such an inquiry; nor is the answer ever likely to be completed. Man, therefore, we may be permitted to call a species; Christian, or Mathematician, we cannot.” (Mill, 1843a, pp. 168-169).<sup>9</sup>

Mill expressed confidence that in particular some high level divisions of biological taxa represent natural kinds.<sup>10</sup> Members of the group, “birds” for example, share innumerable many resemblances. Though we can form many propositions expressing uniformities among members of the kind, we do so via simple enumeration (Mill, 1843b, p. 134). We cannot form inductive laws

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<sup>8</sup>Mill referred to *Kinds* throughout his discussion in what is clearly intended to be the technical sense I explicate as his view of natural kinds. Nineteenth century commentators Read, Towry, and Franklin and Franklin referred to Mill’s views on Kinds as his doctrine of natural kinds. Magnus attributed the historical origin of the phrase natural kind to Martineau’s (1859) review of Mill’s philosophy, and disputes Hacking’s claim that the phrase originated in Venn’s (1866) discussion on Millean Kinds.

<sup>9</sup>Mill recognized that naturalists retain a technical sense of “species”: “By the naturalist, organized beings are never said to be of different species, if it is supposed that they could possibly have descended from the same stock” (Mill, 1843a, p. 169). Note that Mill’s take on the genealogical species criterion does not require strict genealogical continuity: it is enough that the members of the species *could possibly* share common descent. This reflects Cuvier’s “or” clause: “A species, then, includes *the individuals which descend from one another, or from common parents, and those which resemble them as strongly as they resemble one another.*” (Cuvier, 1831, p. 73). The “or” clause was retained by Whewell (1840a, p. 17; 1858, p. 22) and others (e.g. Whately, 1827, p. 307)

<sup>10</sup>The picture is complicated by the idea of classification via series (Mill, 1843b, p. chapter 8). Biological taxa may not form a single hierarchy of progressively more inclusive natural kinds (for example where humans are a kind, Mammals a higher kind that includes humans, Vertebrates a more inclusive kind that includes Mammals, etc.). Mill suggested that major divisions occur at specific points where the “main phenomena” (marked by Sensation, Thought, Voluntary Motion, and so on) of animated life undergo marked advances. Natural grouping by resemblances, as would be done in identifying progressively more inclusive kinds, must be subordinated to the “principle of a natural series” (Mill, 1843b, p. 326).

about resemblances shared within kinds. The reason is that the uniformities linking members of a Millian natural kind are not causal. Millian kinds cannot be investigated via “a system of rigorous and scientific induction,” because:

“The basis of such a system is wanting: there is no general axiom, standing in the same relation to the uniformities of coexistence as the law of causation does to those of succession. The Methods of Induction applicable to the ascertainment of causes and effects, are grounded upon the principle that everything which has a beginning must have some cause or other... But in an inquiry whether some kind (as *crown*) universally possesses a certain property (as blackness), there is no room for any assumption analogous to this.” (Mill, 1843b, p. 126).<sup>11</sup>

Mill discussed the investigation of kinds and natural classificatory science as an operation “subsidiary to induction” (Mill, 1843b, book 4, chapter 7). According to Mill, science is most essentially an inquiry into cases of causation via induction. As scientific investigation proceeds, however, Mill expected that some classes – natural kinds – will resist being fitted into causal laws. Science includes a process of ever-generalizing induction that produces causal explanation, but also investigation of kinds as a separate matter. As scientific investigation proceeds, some proposed kinds will be discovered to be groups whose shared features can be explained via causal property dependence. Yet Mill seemed confident that *some* kinds will be ultimate and united by shared possession of non-reducible properties.

Mill expected that some biological taxa are natural kinds, though investigators had not yet clarified which taxa will be real kinds. In places he claimed that biological systematic nomenclature

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<sup>11</sup>Mill’s phrasing in contrasting laws of succession versus those of co-existence may suggest that he has only temporal causation between events in mind when discussing the justification of claims about natural kinds. In the same discussion Mill (1843b, p. 123) suggested that “the specific properties of the compound are consequent, as effects, upon some of the properties of the elements...” Mill asserted that if this is so, chemical compounds are not natural kinds. The relation between elemental properties and properties of compounds is causal property dependence, not temporal succession between events. Yet the relation of causal property dependence is a relation between cause and effect, and would be justified by the causal necessity that underwrites causation in general.

aims to specify biological species as lowest kinds: “The name *Violataodorata* denotes a Kind, of which a certain number of characters, sufficient to distinguish it, are enunciated in botanical works” (Mill, 1843a, p. 289). If subsequent research revealed that the characters are reducible to a small number of core properties, the proposed species would not form a natural kind (regardless of its reproductive isolation from other recognized biological species). On the other hand, if research revealed that individual plants referred to by the name, *Violataodorata*, belong to multiple distinct natural kinds, the group must be split such that the specific name refers to only one, lowest kind (alternately, the name *Violataodorata* may be abandoned and new specific names coined).

Mill suggested that part of the connotation of biological nomenclatural species names is that they refer to lowest natural kinds:

“The form of the name declares that, happen what will, it is to denote an *infima species*; and that, therefore, the properties which it connotes, and which are expressed in the definition, are to be connoted by it no longer than while we continue to believe that those properties, when found together, indicate a Kind, and that the whole of them are found in no more than one Kind.” (Mill, 1843a, p. 290).

Though this may be a goal of systematic nomenclature, Mill occasionally expressed uncertainty that the groups that naturalists distinguish in practice as biological species will actually meet his natural kind criterion. Already in 1843, Mill speculated that individual organisms are extremely modifiable, and that the differences between individuals within a biological species may be due to interacting causes (Mill, 1843b, p. 131). One of Mill’s more common examples of a natural kind is “crow”. P. D. Magnus (2014, p. 10) pointed out that Mill, when he wants to, utilizes the distinction between technical biological versus common names, and suggested that by “crow” Mill did not intend to specify a particular biological species. Rather, Mill’s use of “crow” is deliberately vague with respect to biological nomenclature, perhaps indicating something along the lines of a genus. It might turn out that the differences between recognized biological species are due to interacting causes, but that the difference between genera are not the consequences of any causal processes.

Mill expressed openness to Darwin's theory, and was clearly not opposed to the possibility that biological species have not always been differentiated along their current lines (Mill, 1875, p. 19; 1972, letter to Hewett C. Watson, Jan. 30, 1869, to Edward Livingstone Youmans, March 1869, to John Elliot Carnes, Dec. 5, 1871). It is unclear why Mill should draw a line between species and genera. If there is in fact a historical story that led to the contemporary groupings of species, the shared properties of each genus could be explained as consequences of the causal transactions that occurred in this historical story. Thus for Mill, biological species and genera may not be kinds, because their shared resemblances may in fact be reducible to consequences of the historical causal story.

If Darwin is correct in asserting that all organisms descend from some small number of ultimate ancestors, no biological taxon could be a Milleian kind. Yet the later editions of the *System* retain statements expressing confidence that biological taxa are natural kinds (Mill, 1875, p. 279). Mill's decision not to revise the later editions of the *System* on this point probably reflected his lack of certainty about Darwin's theory. Mill considered Darwin's theory promising but not proved (Mill, 1875, p. 19). As will be seen shortly (page 37), there may be another possibility that would reconcile the historical common ancestry of biological species with the claim that biological species are Millian natural kinds. The key to this reconciliation is a question that hangs over Mill's theory of natural kinds: if not some form of causal necessity, what is it that unites the members of a natural kind?

### 2.2.3 Justifying Mill's Natural Kinds

What about the structure of the universe underwrites Mill's appeals to "uniformities of co-existence" that are separate from the law of causation? Carveth Read (1877) pressed this question in his analysis of Mill's distinction between uniformities due to causation versus uniformities among natural kinds.

As Read explicated him, Mill held that scientists hypothesize both causal laws and classes of co-existing resemblances:

“For every Law of Causation is the Definition of a Class of Causal Instances; and every Definition of a Natural Kind is a Law of Co-existence.” (Read, 1877, p. 344).

Read described Induction as a “test” of the constancy of the relations so predicated:

“The Induction of relations of Succession is governed by the Law of Causation; the Induction of relations of Co-existence is aided (much less effectively) by the doctrine of Natural Kinds.” (Read, 1877, p. 345).

Read then claimed that:

“...complete generalisation requires that one should be reduced to the other; and, as it is, we cannot hesitate to endeavour to reduce Co-existence to the effect of Causation.” (Read, 1877, p. 346).

Read stated that his motivation was to avoid a “perpetual duality of conception” – that is, he sought to simplify all invariable uniformities to one form. According to Read, the doctrine of Natural Kind is “very inferior” to the Law of Causation as an instrument of investigation, and therefore we should work to reduce uniformities of co-existence to regularities governed by causation. Underlying Read’s motivation to simplify is a dissatisfaction with the Laws of Co-existence. Absent any explicit principle to justify the Laws of Co-existence, natural kinds seem to be residual uniformities that resist scientific explanation. Each kind represents an invariable grouping of properties apparently as a matter of brute fact that we can no more explain than the initial configuration of matter in the universe (Mill, 1843b, p. 44). Read did not describe how the reduction of laws of co-existence to those of causation was to be accomplished.

Franklin and Franklin (1888) suggested a way to causally ground Millian natural kinds. The distinction between natural kind and non-natural kind can be made based on the type of causal connection that unites the group. Mill was on the right track, Franklin and Franklin claimed, in denying that natural kinds are united by a core group of properties on which all other shared

properties depend directly. Instead, kinds share very many properties due to “a certain community of origin” (Franklin & Franklin, 1888, p. 84). The members of a natural kind share some common historical past, and the very many resemblances among kind members are due to this past causal story. Natural kinds share resemblances that are not causally dependent on other properties, but that are linked by causal history. Franklin and Franklin accepted Mill’s prescription that kinds share very many properties that do not directly depend on each other, but denied that sharing of causal history amounts to property reduction.

Franklin and Franklin stressed a negative criterion for handling natural kinds. While they *expect* that a common origin will be discovered for the members of a kind, the important point is that the kind’s shared properties cannot be reduced via direct causal dependence between properties. The expected common origin story is rarely recognized when natural kinds are first identified, and the historical story may remain unknown. Indeed, Franklin and Franklin claimed that their negative prescription – natural kinds’ shared properties are not causally reducible – suffices to distinguish kinds even if the reader disagrees with their positive claim, that a common origin unites kind members. Franklin and Franklin offered chemical elements as an example:

“...the fact that all portions of matter which possess a few of the properties of sodium do actually possess all the other properties of sodium forces upon us the conviction that either the qualities or the objects have a real connexion with each other. If the former is the case, the properties of sodium are deductions from its molecular constitution; if the latter, then sodium is in a very valid sense a Natural Kind – something very different from an arbitrary and “merely intellectual” class: and this, whether one agrees or does not agree with the present writers in regarding the connexion between the objects to reside in a certain community of origin.” (Franklin & Franklin, 1888, p. 85).

Franklin and Franklin were unsure about whether all sodium objects in fact share a common origin.

In 1888 the picture seemed clearer in the case of zoological species:

“... in the case of the animals forming a species, it would be preposterous to suppose that all the common qualities might be explained deductively from a few of them. These, then, form a Natural Kind, in the sense in which we have used the term; and, in this case, community of

origin has been sufficiently shown to be the true ground of the classification.” (Franklin & Franklin, 1888, p. 85).

Many of Mill’s commentators expressed approval of some version of Mill’s criterion of reducibility. In an otherwise highly critical response to Mill’s *System*, Whewell wrote:

“I consider also that the recognition of *Kinds* (i. 166) as classes in which we have, not a finite but an *inexhaustible* body of resemblances among individuals, and as groups made by nature, not by mere definition, is very valuable, as stopping the inroad to an endless train of false philosophy.” (Whewell, 1849, p. 85)

Mill’s negative criterion picks up on an important element of systematic practice. Systematists are now and were in Mill’s day highly concerned about the independence of characters. The challenge is to identify whether apparently distinct characters in fact represent independent evidence of use in natural classificatory claims. The issue of character analysis recurs throughout this dissertation, most especially in chapter six. It may be that Mill’s cognizance of systematic practice was a source for his view on natural kinds. If so, Mill took an individual element of systematic practice and extended it to a view of natural classificatory science in general. Classifiers attempt to identify causally independent properties, and then discover that objects in the world can be grouped on the basis of bearing clusters of independent properties. Indeed, seemingly as a brute fact, the clusters would turn out to include inexhaustibly many independent properties.

Commentators found much less satisfying Mill’s idea that kinds are united by non-causal uniformities. On the one hand, the worry is that Mill’s view greatly reduces the domain of application of scientific explanation. On the other hand, advances in biological and chemical sciences suggested that Mill’s doctrine of natural kinds could not apply to many entities at all. Uniformities among biological entities could be, in principle, explained by causal laws invoking evolutionary processes and historical origins. The uniform properties of chemical elements could be, in principle, explained by causal laws invoking atoms and other microscopic entities and processes.



The suggestion to distinguish properties shared due to causal dependence versus common historical origin will be further explored in chapter five.

### 3.0 WHEWELL'S PHILOSOPHY OF SCIENCE

In this chapter I present William Whewell's critique of Mill's empirical methods as insufficient to address the core of scientific method. In section 1 I show how Whewell's philosophy of science addresses the problem by analyzing the conceptual component of scientific discovery. Section 2 addresses the problem in classificatory science, showing how Whewell developed the concept of consilience in the domain of biological systematics. Systematics appears to have been a major source for the development of Whewell's consilience, which in turn would influence Charles Saunders Peirce and subsequent proponents of inference to the best explanation. Section 3 addresses Whewell's attempts to justify the use of consilience in the domain of systematics by reference to the causal structure underlying the natural classification of organisms. Whewell's description of teleological reasoning is philosophically separable from his arguments about Design, and matches up with his contemporary scientific sources who did not appeal to Design. However, Whewell did appeal to divine final causes as undergirding the natural classification, and to Design as justifying the use of teleo-functional reasoning. Whewell's account of consilience in systematics will be reformulated in chapter 6 in terms of an alternate causal structure developed in chapter 5.

### 3.1. WHEWELL'S INDUCTION AND CRITIQUE OF MILL

#### 3.1.1 The Role of Concepts in Science

Chapter 2 introduced a challenge: how to identify relevant properties through which to group entities when postulating causal laws? Faced with indefinitely many potential ways of grouping phenomena, how do we identify those resemblances that reliably mark causal transactions? Mill's response is that we can rely on experience. According to Mill, science progresses by considering different potential groupings (A, B, C, etc.) in light of the four or five methods of experimental inquiry (Mill, 1843a, book 3, chapter 8). These methods investigate whether causal necessity obtains, addressing either this question directly or weaker conditions ultimately aimed at this question.

Whewell criticized Mill's scheme of experimental methods in his *Of Induction, with Especial Reference to Mr. J. Stuart Mill's System of Logic* (Whewell, 1849). Mill offered few examples of the use of his methods in science, and Whewell complained that Mill did not sufficiently analyze the examples that he did offer (Whewell, 1849, p. 46). Thus Whewell could see no response, from Mill, to a serious problem: the experimental methods presuppose that the phenomena are already neatly delineated and grouped as types denoted A, B, C, and so on. Yet, Whewell claimed, achieving this grouping is itself the hard part of scientific inquiry (Whewell, 1849, p. 44). Whewell invoked the authority of Herschel (1831, p. 183) making a similar complaint about Bacon's *Prerogatives of Instances*, which "have a great resemblance" to Mill's methods (Whewell, 1849, p. 45). Joseph (1916, p. 441) would also launch such a critique of Mill, and the general problem would recur in twentieth century philosophy of science as a central problem for accounts of induction (chapter 4).

Whewell's philosophy of science clarifies the reason that the activity of grouping is difficult, and what a solution to the general problem requires. On Whewell's system, theoretical content

becomes embedded in the grouping of phenomena in a way that subsequent empirical testing cannot disambiguate. Mill's methods may test the regularity of proposed causal laws against the world, but the way in which the laws are formed (the grouping of phenomena) already presupposes background assumptions about what are causally relevant properties and entities. These assumptions are embedded in particular scientific theories and research traditions. In contrast to Mill, Whewell grappled with this challenge directly, and addressed the theory-ladenness of observation as a critical challenge for philosophy of science.

Whewell addressed this theory-ladenness from the very beginning of his system, arguing that there is an inevitable and necessary ideational contribution from the observer in even the most basic observation. In Whewell's terms, scientific knowledge requires contributions from both *sense* and *ideas* (Whewell, 1840c, p. xvii). Every perception requires an act of the mind, an application of ideas to formless sensation (Whewell, 1840a, p. 2; 1840c, p. 36).<sup>12</sup> Every observed "fact" already includes a contribution from ideas in the mind. Whewell claimed that the apparent distinction between fact and theory is really about our consciousness of the ideational component: facts involve ideas unconsciously, while theory is a matter of conscious application of ideas to facts (Whewell, 1840c, pp. 22, 24). This distinction is not absolute (Whewell, 1840c, p. 25), but Whewell did maintain a distinction between *sense* (by which impressions are produced, without an active contribution from the mind) and *idea* (which the mind actively, constantly, inevitably imposes on the matter of sensation).

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<sup>12</sup>Whewell wrote that "Matter cannot exist without Form", and also that "without ideas, sensations have no form" (Whewell, 1840a, pp. 1-2). He held that the concept of matter can be distinguished from that of form, though matter never exists without form. Similarly, the concept of sensation absent form can be distinguished from that of perception as we experience it. Our perceptual experiences are always through informed ideas – the act of perception entails the imposition of form on sensation (Whewell, 1840c, p. 36).

Whewell's induction is an act of super-inducing an idea upon observed facts (which themselves already involve ideas), thereby connecting the facts to produce new facts, potentially of successively greater generality (Whewell, 1840c, p. xl). Whewell analyzed the sciences in terms of *Fundamental Ideas*, such as space, time, cause, and resemblance (likeness), which are gradually clarified and refined as more specific scientific concepts. For example, the concepts *matter* and *force* are each refinements of the Fundamental Idea of cause. Whewell argued that the selection of the correct idea, and the correct refinement of the idea, is crucial to successfully approaching each problem. Different research traditions, at different times, will involve different Fundamental Ideas and different modified conceptions thereof. For Whewell, the progress of inductive science consists in selection and clarification of concepts together with application of these concepts to observed facts. The most essential task of science is the formation and reformation of the basic categories in terms of which the scientist addresses the phenomena, and this task is logically prior to the proposal and testing of law-like regularities.

Whewell's system stands in stark contrast to Mill's thoroughly empiricist approach. In Whewell's philosophy, the Fundamental Ideas are innate. They are not gathered from experience but rather compose our conceptual apparatus itself. Whewell's use of "Idea" differs radically from the empiricist usage in which an "idea" is a record of an experience – a copy either directly of a sense impression, or of some mental experience that ultimately derives its content from sense impressions. For example, Mill described "resemblance" as a relation between ideas that is not itself analyzable. We perceive "resemblance" as a "feeling" that arises as a result of our experience of the world: something in the world acts upon our sensory apparatus in such a way that the feeling of resemblance is generated in our minds (Mill, 1843a, p. 90). We can make propositions asserting that a resemblance relation obtains among the phenomena that caused the experience. By contrast, Whewell describes resemblance as a Fundamental Idea that we actively superimpose upon the

material of sensation. By application of the idea of resemblance, we collect individual objects into kinds about which we can make general assertions (Whewell, 1840c, p. 463).<sup>13</sup> Indeed, Whewell considers the condition, that groupings must render general propositions possible, a criterion that aids classificatory scientists in identifying relevant resemblances. The task of systematics substantially consists in the development of further criteria to guide the application of the idea of resemblance (Whewell, 1840c, p. 468).

Both Mill and Whewell thought about resemblance and classification as fixing the propositional framework for science in general. This point would crop up throughout the history of biological classification, for example in Gilmour's (1940) oft-cited article in *The New Systematics* (Huxley, 1940) that the purpose of scientific classification is to enable biologists to make inductive generalizations concerning the classified entities. The possibility of such generalizations depends on the scientist's ability to convey information through general propositions that cite identified and named entities. In addition to this task in general science, Whewell discussed classification as the goal of a set of scientific disciplines, which he dubbed the classificatory sciences.

In both general science and the classificatory sciences, a central challenge is, upon the basis of what resemblances are we to form scientific groups? In keeping with what they saw as the general purpose of classification, both Whewell and Mill held that scientists are guided by the condition that general propositions must be possible. Though he knew that this condition alone would not suffice to select resemblances that delineate meaningful groupings, Whewell held that the condition would rule out many trivial groupings:

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<sup>13</sup>Whewell uses "Likeness" interchangeably with "Resemblance" when referring to this Fundamental Idea. The terms are not consistently capitalized, but there are passages where each clearly indicates the same Fundamental Idea associated with the Classificatory Sciences. I have found no particular usage patterns across Whewell's works.

“...for instance, no one would speak of a class of shrubs defined by the circumstance of each having a hundred leaves: for of such a class no assertion could be made, and therefore the class could never come under our notice.” (Whewell, 1840c, p. 471).

The problem with trivial groupings is not simply that they would not come under our notice, but that they do not correspond to anything meaningful in the world. The criterion that general propositions must be possible is diagnostic because the general propositions that scientists find meaningful are those that support generalizing inductions about the world.

The development of a successful classificatory framework enables empirical generalizations that Whewell termed *Laws of Phenomena*. Scientific discovery then proceeds to investigation of *Laws of Causes* that describe interactions among the delineated entities. Like Mill, Whewell (1847a, p. 75) accepted Hume’s claim that nothing in the external world can produce a sensation (and thence a conscious perception) answering to the idea of causal necessity (Whewell cited the *Enquiry Concerning Human Understanding*, section 7 part 1). Experience of the external world cannot produce the idea of causation with content beyond mere regularity. Again like Mill, Whewell rejected Hume’s skeptical conclusion. Whewell held that we do, in fact, have the idea of necessary causation, and this idea answers to something real in the world. Unlike Mill, Whewell argued that our ideas of necessity, including causal necessity, derive from Fundamental Ideas. Fortunately for us, according to Whewell, our Fundamental Ideas do in fact line up with reality. This is because both the external world and our human minds were created in accordance with God’s plan.<sup>14</sup> The structure that our Fundamental Ideas impose on our sensory perceptions corresponds with the real, created structure of the world. Laws of Causes derived from Fundamental Ideas are not just psychological constraints

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<sup>14</sup>Rieppel (2010) noted that Rensch (1968), following (Ziehen, 1934), proposed that our ideas about classification correspond to really existing relations in the world because of evolution. On this line, it is natural selection rather than God that adapted the human mind to line up with the external world.

that we impose on our perceptions; the Fundamental Ideas capture real entities and relations in the external world.

Mill cited favorably Whewell's distinction between Laws of Phenomena and Laws of Causes in defending the existence of real causal necessity against Comte's skepticism (see page 20). Clearly Mill did not intend to take on board Whewell's ideational ontology or theological justification for our knowledge of causes. Mill analyzed causal interactions as regularities in which causal necessity obtains, but did not think that scientists could analyze the nature of causes directly. Milleian causal laws are simply statements of unconditional regularity. In contrast, Whewell held that scientists develop distinct ideas about different types of causation. In a Whewellian framework, the refinement of scientific concepts includes development of distinct types of causal laws. The fact that scientists develop different types of causal law complicates the problem of categorizing phenomena, that is, the "hard part" of science. The case of Kepler and Newton illustrates the distinctions between Whewell's Laws of the Phenomena versus Laws of Causes, as well as what Whewell took to be the problem with Mill's views on the formation of causal laws.<sup>15</sup>

### 3.1.2 Concepts and Causes in Kepler's Discovery

Mill claimed that Kepler's discovery that planetary orbits are elliptical is just a descriptive fact that Kepler noticed in the world. Mill compared Kepler's scientific process to the method followed by a sea captain who sails around a newly sighted body of land, making observations as he sails (Mill, 1843a, p. 357). The captain discovers the fact that the body of land is an island simply by taking

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<sup>15</sup>The discussion occurs in (Whewell, 1840c, pp. 41-48); (Whewell, 1849, pp. 18-35), which is reproduced in the third part of the third edition of the *Philosophy of the Inductive Sciences* (= *Philosophy of Discovery*) (Whewell, 1860, pp. 247-257); and (Mill, 1843a, pp. 356-369), expanded in subsequent editions (Mill, 1865b, pp. 325-342).



stock of his series of observations. Similarly, claimed Mill, Kepler's discovery that the orbit of Mars is an ellipse is simply a descriptive summary of a series of observations of the position of Mars.<sup>16</sup> Such a description involves direct observation but not induction: there is no causal necessity, no general statement asserting a causal transaction (that is, no Millian causal law). On Mill's view, Newton's law of gravity asserts that a causal necessity obtains between entities meeting the general name "objects with mass". Newton's discovery is a causal law that is of the very same sort as all other scientific laws, though at a very high level of generalization.

On Whewell's view, Kepler formulated phenomenal laws that describe the behavior of entities in the world. This discovery involved induction far beyond simple description of the phenomena. According to Whewell, Kepler added something to the positional observations: there was an act of mind, the formulation of a conception, above and beyond mere description. While even the most trivial act of observation involves a mental act (e.g. seeing a tree as a tree - Whewell, 1847a, p. 467), Kepler's discovery refined the way in which scientists like Newton perform such mental acts.

In Whewell's account, Kepler refined the conception *planetary motion* and devised three laws that describe the behavior of the phenomena to which *planetary motion* applies in the world. Newton advanced the concept of *centers of force* as an aspect of the Fundamental Idea of *cause*. Newton then applied the concept, center of force, to the phenomenal laws developed by Kepler which reference the concept of planetary motion. All of these conceptual advances crucially enabled Newton to

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<sup>16</sup>In the course of an insightful discussion, Snyder (2006) summarized some obvious problems with Mill's description. Kepler could not directly see the motions of Mars alone relative to the sun; he had to account for the motions of the rotating, revolving earth from which his observations of Mars were made. Moreover, Mars' orbit is sufficiently close to circular that even were a bird's eye view possible, the human eye would not detect the divergence from circularity. Mill's claim is that the relevant mathematical transformations amount to description without induction and without any reference to causation.

discover gravitation as a new type of cause, a new Law of Causes. Such a discovery is the most difficult and in some ways the most perplexing kind of scientific advance. Whewell described the search for new causes as “feeling in the dark for an object which we cannot find” (Whewell, 1847b, p. 101). Whewell could not fully explain how Newton is able to make the crucial step of conceiving a new kind of cause. Yet Whewell was clear that Kepler’s refinement of the concept of planetary orbits necessarily preceded and prepared Newton’s “feeling in the dark”.

Though Whewell frequently praised Newton, he also cautioned against imitating Newton in other sciences whose conceptual structures had not been sufficiently developed. Discovery of Laws of Causes can only come after discovery of Laws of Phenomena (Whewell, 1847a, p. 322). Causal laws involve conceptions that must be developed from the Fundamental Idea(s) appropriate to the phenomena and question at hand. Kepler’s phenomenal laws of motion are a conceptual refinement, fit to the appropriate domain, of the Fundamental Ideas of space and time, ideas refined through the advance of the science of astronomy. Had Newton embarked upon his search using insufficiently developed concepts, or concepts not appropriate to the task, his search would not have succeeded. For example, had Newton conceived *planetary motion* in terms of nested epicycles, it is unlikely he would have been able to discovered gravity as a cause.

Whewell is quite clear that we should not expect future discoveries of laws of causes to cite the concept of “centers of mechanical force” (Whewell, 1847a, p. 98). Rather, Whewell expected that distinct sciences will discover completely different types of cause. Perhaps molecules can be viewed as microscopic shapes, bouncing about and interacting just like Hume’s billiard balls. But what is the force that binds such molecules together? Such a force will somehow underlie the observed external properties, but chemical affinity itself seems fundamentally different from mechanical force causation (Whewell, 1847a, p. 99). Whewell also cited heat as likely involving a different form of causation (Whewell, 1847a, p. 184). The nature of heat may, according to Whewell,

be a kind of fluid rather than molecular motion. He discussed causal relations involving heat in terms of equal reactions, proportionality of effect size, and so on. His point is that scientific investigation must proceed without the assumption that all causal relations take the form of simple “mechanical action” (Whewell, 1847a, p. 183). Whewell especially (Whewell, 1847a, p. 580; 1847b, p. 99) expected that the kind of causation described by the Idea of Vital Powers, which concern the nature of *life*, cannot be reduced to transactions of mechanical force.

As will be seen in chapter 5, Whewell himself attempted to develop the concept of *historical cause* in his own practice of historical science. In clarifying the distinction between historical cause and the types of permanent causes targeted in experimental science, Whewell provided conceptual resources for understanding systematics as a historical enterprise.

Whewell took umbrage at Mill’s portrayal of Kepler’s discovery as “mere description” absent any direct contribution to causal reasoning. The point is larger than the particular problems with Mill’s description of the case. Whewell’s criticism reflected his philosophy of science as progressive refinement of concepts. Whewell noted that Kepler tried nineteen different figures before trying the ellipse (Whewell, 1847b, p. 42). Familiarity with each of these different figures was prerequisite: Kepler could hardly have discovered that Mars’ orbit is elliptical, had Kepler lacked an understanding of what an ellipse is (Whewell, 1849, p. 29). Whewell’s point is that Kepler actively applied previously refined concepts to the data, rather than simply collecting descriptive data points.

This point would be reiterated in many subsequent accounts of scientific discovery (e.g. Hanson, 1958) stripped of Whewell’s theological justification for the match of concepts to the world. There are, in principle, infinitely many mathematical figures that Kepler might have tried (had he the necessary education and familiarity with each figure). This point holds with respect to Mill’s four methods in general: there are infinitely many possible “letters” that we might try out, in formulating a proposed Millian causal law. According to Whewell, producing the set of “letters” for

trial is the crucial step in scientific induction, and addressing this step is a core element of Whewell's philosophical apparatus.

The decision to treat the orbit problem as a shuffling through geometrical figures provided Kepler a principled approach to generating hypotheses. In the case of Newton discovering a new kind of cause, it is unclear what kind of an entity might even count as a "letter". We cannot, according to Whewell, simply abstract the letters directly from raw experience: an active Idea is required. Mill has omitted the crucial step of refining and selecting ideas that group the phenomena in such ways that hypothesized Millian causal laws can be tested. Whewell's complaint is that Mill's gave no guidance as to how the concept formation and application is to be achieved, so that his methods are not useful in the main, difficult part of science. As will be seen in chapter 4, subsequent philosophy of science largely avoided addressing this "hard part" of science. Accounts of explanation suffered a cycle of relevance objections that traded on the assumptions of Mill's framework that generate Whewell's hard part objection.

Moreover, properties that are causally relevant in the perspective of one branch of science may not be relevant with respect to another science. The names *ellipse*, *planetary orbit*, and *object with mass* connote relevant properties with respect to physical astronomy, but not with respect to physiology.<sup>17</sup> In Whewell's system, the formation of concepts may proceed using different notions of cause (mechanical force, vital power, chemical affinity, and so on), and the properties that are relevant with respect to one type of cause may not be relevant with respect to another type of cause. Newton could not have discovered a new kind of cause by considering potential relations between entities grouped by general names connoting chemical or physiological properties.

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<sup>17</sup>Except perhaps in some extremely indirect way, and dealing with cases that would not have been relevant to the physiological research program in Whewell's day: for example speculation about what might happen to John Glenn during a prolonged spaceflight, about colonization of planets by micro-organisms embedded in meteors, and so on.

### 3.1.3 Concepts and Causes in Classificatory Science

In developing his philosophy of science, Whewell was undoubtedly influenced by his experience with classificatory science. He was appointed Chair of Mineralogy at Trinity College in 1828 but found the science of classifying of minerals to be in a highly disorganized state (Whewell, 1828). He wrote in particular about the failure of mineralogical classification on the basis of crystallography, in contrast to the success achieved in zoology and especially in botany.

The problem, Whewell argued, was premature causal speculation. It is an ultimate goal of crystallography to discover Laws of Causes that explain crystalline form and why there are the groups of crystals that there are. However, before Laws of Causes can be posited, further progress in the relevant phenomenal laws was required. Attempts to derive phenomenal laws had gone astray because they had been developed from inappropriate ideas owing to over-eagerness for causal speculation. Nineteenth century crystallographers were not in a position to advance coherent causal explanatory hypotheses of crystalline properties in terms of Millian laws, because the necessary elemental vocabulary (the “letters”) was lacking. Confusion had arisen because it was erroneously assumed that crystalline properties would be explained using the elemental vocabulary of chemistry. Attempts to form classifications of crystals based on the proportional mass of constituent elements did not proven useful. The problem is that such explanatory and classificatory attempts are motivated by the presumption that elemental composition caused the crystallographic properties in a straightforward manner that would be reflected in the natural classification of crystals.

Whewell accepted the presumption that the macroproperties of crystals – their geometrical forms, optical properties, cleavage, and so on – must be caused by the crystals’ microstructures, because, he reasoned, there is nothing else that *could* explain the macroproperties (Whewell, 1847a,

pp. 401, 403, 460). However, phenomenal laws, including ultimately a natural classification of crystals, must be formed through consideration of the properties essential to the nature of the classificatory entities themselves. These properties are approached through the ideas of *symmetry* and *polarity* (Whewell, 1847a, p. 459). Whewell allowed that it is quite possible that the crystals are formed of arrangements of atoms, whose elemental nature and spatial arrangements cause the macroscopic properties. He argued, however, that workers have focused narrowly on the elemental nature of the atoms and ignored the crucial aspect of the atoms' orientations.

Whewell expected that the causal structure underlying crystalline forms would not be comprehensible simply through understanding of the causal laws and concepts of mechanics such as inertia, mass, and velocity. With respect to the explanatory concerns of crystallography, relevant causal properties will be those related to the ideas of symmetry and polarity. Progress in crystallography requires refinement and application of concepts pertaining to these properties, and crystallography would contribute to the natural classification of minerals through understanding of these properties.

The natural classification of crystals would prove to be *compatible* with chemical laws. Indeed Whewell claimed that the agreement of a classification formed on the basis of crystallographic properties with a classification based on chemical properties would be powerful evidence supporting the combined classification of all minerals. Elemental composition, polarity, symmetry, and perhaps other of Whewell's Fundamental Ideas might have been methodologically important in advancing toward the natural classification, and may each serve different explanatory roles respecting different causal processes relevant to mineral classification.

The agreement of classifications formed on the basis of distinct ideas, approached through distinct scientific research programs, would prove crucial to Whewell's philosophy of science. The concept of *consilience* will be developed throughout the remaining chapters of the dissertation. I

introduce the concept in the next section through Whewell's application of consilience in the classificatory sciences in a somewhat refined form: *natural affinity*. Whewell developed the idea of natural affinity in the context of botanical systematics, and to the extent that ensuing accounts of abduction (chapter 6) are indebted to Whewell's account of consilience, the debt can be traced to the philosophical study of systematics.

### 3.2: WHEWELL ON NATURAL AFFINITY

#### 3.2.1 Goals and Problems of Classification

The term *natural affinity* recurs throughout pre-Darwinian systematics, yet there has been insufficient historical work on what the concept meant to different authors. Most frequently, affinity is described as referring to the real relationships described by the natural classification, whatever the basis of the natural classification is taken to be. For example, Ospovat (1981) identified affinity as the relationship evidenced by Owen's (both pre- and post- evolutionary) concepts of "homology". Ospovat (1981) identified Geoffroy as Owen's source for the distinction between relationships of affinity and mere analogy. Snyder (2006, p. 160) synonymized natural affinity with "essence" or "some underlying essential commonality" on the assumption that the natural classification delineated objects on the basis of their metaphysical essences. Snyder (2006, p. 157) stated that in 1825 Whewell converted to the "natural classification system" which grouped entities on the basis of natural affinity. Snyder apparently intended the term *natural affinity* to indicate an ontological point (affinity refers to something real in the world), a normative point (it provides a superior basis for classificatory science), and a historiographic point (its use was a new method in classificatory science,

linked to a concern to discover the natural system). However, for Whewell, *natural affinity* held a very specific methodological meaning that has largely been overlooked.<sup>18</sup>

Whewell explicated the idea of natural affinity via what he saw as its gradual clarification and use in historical attempts at natural classification. Whewell adopted Candolle's three-stage division of this history – indeed, Whewell's historical analysis in the *History of the Inductive Sciences* (1837c) and *Philosophy of the Inductive Sciences* (1840c) closely follows Candolle's (1813) *Théorie Élémentaire de la Botanique*. Whewell argued that each of Candolle's stages is characterized and its relative success explained by the use or disuse of Fundamental Ideas beyond mere likeness (resemblance). Success in classificatory science is proportional to the use of natural affinity together with other concepts as they are revealed to be relevant to understanding the classificatory entities. Success is achieved through progressively more advanced approaches to the problem of identifying relevant resemblances.

From the very start, the question is: what resemblances should be used in classification? Even the most preliminary practice of classificatory science, the development of terminology, is riddled with assumptions that reflect this challenge:

“But in fixing the meaning of the terms, at least of the descriptive terms, we necessarily fix, at the same time, the perceptions and notions which the terms are to convey; and thus the Terminology of a classificatory science exhibits the elements of its substance as well as of its language.” (Whewell, 1847a, p. 481).

Development of a Terminology enables scientists to speak about the same entities – objects and parts of objects – and also to express the same qualities of features. For example, *apple green* may acquire a fixed, technical meaning as part of a Terminology, regardless of the color of particular apples and regardless of whether any given worker has ever seen an apple (Whewell, 1840c, p. 464).

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<sup>18</sup>The most recent substantive published account that I have found is in Bather (1927).



With the proper training, different workers can then understand the same shade of green without needing firsthand visual experience of the original object of description or a colorized reproduction (for example, a drawing of a green apple). Whewell claimed that the fixation of terms serves to “fix our attention upon the resemblances which it is proper to consider” (Whewell, 1847a, p. 485). By continued study of the objects themselves, classificatory workers learn what kinds of features can vary while others remain the same. The terminology encodes this knowledge by fixing what kind of resemblances the worker can conveniently express. Whewell offered an example: botanists have developed the terms *bilobate*, *bifid*, *bipartite*, and *bisect* to describe the extent to which a leaf is divided by a central notch.

“...a leaf is called *bilobate* when it is divided into two parts by a notch; but if the notch go to the middle of its length, it is *bifid*; if it go near the base of the leaf, it is *bipartite*; if to the base, it is *bisect*.” (Whewell, 1847a, p. 484).<sup>19</sup>

The availability of these terms draws an aspect of leaf form to the worker’s attention and suggests that the feature be considered with respect to one of these four categories, rather than by some other category or an exact numerical measurement of the notch.

Fixation of terminology already implicitly affects decisions about weighting resemblances by delineating what might count as a character and whether the character counts as a resemblance in some observed case. A worker could choose to describe five, or ten, or some other number of categories for the extent to which a notch divides a leaf. This decision may determine whether a given configuration counts as a resemblance between two specimens or a difference. The decision may be a matter of scientific tradition rather than conscious deliberation, once a terminology has

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<sup>19</sup>Whewell cited Candolle (1813, pp. 455-456) for these terms. Candolle actually presented more generalized terms that cover notches in any structure (not just leaves) and that may divide the structure into more than two parts. For example Candolle offered the term “Partagé (partitum)”, which can be extended to *bipartite*, *tripartite*, and so forth. For Whewell’s purposes and mine, the salient feature in the example is the comparative depth of the notch or notches.

become fixed within a scientific research program. The terminology is open to subsequent revision, however, in the event that the properties describable by means of the available terms do not aid in identification of properties relevant to the formation of phenomenal or causal laws within the research program.

Whewell then turned the focus to what he calls the plan of the system. Systematists explicitly compare and weight characters to form a plan through which entities will be classified. Systematists decide which characters will form the basis of groups and how much importance to accord each resemblance. Whewell here appealed to the overarching requirement that scientific classification render general propositions possible.

“The object of a scientific Classification is to enable us to enunciate scientific truths: we must therefore classify according to those resemblances of objects (plants or any others), which bring to light such truths.” (Whewell, 1847a, p. 486)

A scientific classification must line up with the whole of scientific knowledge: we must not found groups on trivial resemblances that are likely to cut across the general laws of ordered nature. This prescription is of little direct use in developing the plan of the system, however. This is because for Whewell, scientific classification plays its proper role in discovery prior to the elucidation of general laws:

“...the task of classification of plants was necessarily performed when the general laws of their form and nature were very little known; or rather, when the existence of such laws was only just beginning to be discerned.” (Whewell, 1847a, p. 487).

Classification antecedes discovery of general laws not in a contingent, historical sense, but necessarily, according to Whewell’s philosophy of science. The Laws of Phenomena by which classificatory scientists group entities necessarily precede investigation of more generalized laws that refer to the forms and natures of the classificatory objects.

When scientists don’t yet know these general laws, it is difficult to tell what kinds of classifications are more or less likely to cut against the general laws. Whewell suggests that scientists

can rule out some classifications that are manifestly arbitrary, such as Dioscorides' division of plants into *aromatic*, *esculent* [edible], *medicinal*, and *vinous* (Whewell, 1847a, p. 487). The properties used in this division derive from consideration of how humans make use of the classificatory entities. Appropriateness for human uses do not line up directly with properties that we expect will enable us to describe the basic causal structure of the world.

Whewell's prescription about forming general propositions rules out classifications that fail to support successive generalization of inductions. Dioscorides' division might support generalizations at a trivial level pertaining to human use, but we cannot extend such claims to any higher level of generalization. Any potential extension is a reducible consequence of the initial property of human use. However, what is important in Whewell's system is not that the property, "human use", is a consequence rather than a cause of other properties. For Whewell, the problem is that we do not expect the property to line up with causal laws about the proper domain, the natural classification of plants that reflects an underlying causal story distinct from human uses. Dioscorides' classification is to be abandoned because it does not support successive generalization with respect to laws in any theory pertaining to this natural classification.

Likeness alone – recognition of true resemblances – is not sufficient to fully solve "the selection of the resemblances which should be taken into account" (Whewell, 1847a, p. 487), so that we can construct classifications that do not contradict general laws about the nature of the classificatory objects. Identifying this general problem is easy in trivial cases where a proposed principle of likeness is known to be irrelevant to the objects' natures. Whewell argued that in more difficult cases the solution must come from Fundamental Ideas other than likeness. To develop a plan of the system, the most important Fundamental Idea is *natural affinity*, which appeals to other Fundamental Ideas relevant to the nature of the classificatory objects.

Whewell described early naturalists' method as blind trial (Candolle's "tâtonnement" - 1813, p. 67). Naturalists described various definite resemblances among parts of objects, but in place of a structured method of comparing different resemblances, used only an "obscure feeling of a resemblance on the whole, an affinity of an indefinite kind" (Whewell, 1847a, p. 500). Following Candolle, Whewell cited passages from Magnolius and Linnaeus and noted that these workers were able to identify many natural groups despite the absence of a principle plan of the system. In Candolle's (1813, p. 70) telling, Linnaeus claimed that the formation of natural groups is the goal of botanical classification, but declared that no systematic consideration of characters can achieve this task.

According to Whewell, Linnaeus' pessimism and failure to form a successful plan of characters resulted from his failure to apply Fundamental Ideas other than likeness (Whewell, 1847a, p. 500). The method of blind trial involves clear use of the Idea of likeness only. Other Ideas may influence the worker but not in any principled, systematic way, and perhaps only unconsciously. Whewell argues that Linnaeus and Magnolius' successes in identifying particular Natural groups owe to the unconscious involvement of Ideas other than likeness in the obscure, indefinite feeling of "affinity".

Whewell next described a method that he called *general comparison* (de Candolle, 1813, p. 70; Whewell, 1847a, p. 501). The central example is the method that Adanson described.<sup>20</sup> Adanson proposed to devise many artificial systems, each founded upon consideration of some one part of the organisms to be classified. The method proceeds to accept the classification that is best supported in a comparison of all these artificial systems.

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<sup>20</sup>As Mayr (1982, p. 194) pointed out, Adanson applied the method only after having formed hypotheses of genera and families. It is thus historically misleading to present Adanson as having actually applied entirely artificial character schemes to individual organisms as the initial step in classification.

Whewell analyzed the method of general comparison as an attempt, by numerical replication of consideration of resemblances, to devise a plan of the system that weighs resemblances objectively without reference to ideas beyond likeness. Analyses of Adanson's proposal to attain objectivity through numerical rigor would be cited as historical precursors to twentieth century numerical taxonomy (Sneath, 1964). Whewell reiterated Candolle's objections to the numerical method, and the same objections would be reiterated in twentieth century debates about numerical taxonomy. Initial formulations of numerical taxonomy sought to introduce mathematical rigor into taxonomic analysis, and to identify and eliminate circular uses of evolutionary theory in systematics. The program hardened, however, as its proponents sought to exclude any appeals to evolutionary theory at all in systematic inference.<sup>21</sup> The evolutionary implications of taxonomy were regarded as a secondary project to be implemented after taxonomic hypotheses had been developed and well-confirmed by numerical methods. Candolle's criticisms recur as central objections to this hardened version of numerical taxonomy.

Candolle objected that it is not possible to know all the parts of the organisms and all the possible points of view under which the parts might be considered (Whewell, 1847a, p. 503). If a misleading subset of the total number of parts and possible characters of parts is considered, Adanson's method may not generate convergence on the correct natural classification.<sup>22</sup> Yet there is

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<sup>21</sup>See p. 418 of Sneath and Sokal (1973). Sneath and Sokal described the historical development of numerical taxonomy in terms of sharpening criticisms of what they see as fatal flaws in phylogenetic systematics.

<sup>22</sup>The problem recurs for the hardened version of numerical taxonomy that aimed to produce phenetic classifications. Sokal (1967, pp. 18-19) acknowledged that numerical taxonomists sought operational procedures to estimate all observable characters of organisms: "Phenetic relationship has been defined as "...arrangement by overall similarity, based on all available characters without any weighting..." (Cain & Harrison, 1960, p. 3), "...without any implication as to their relationship by ancestry" (Sokal & Sneath, 1963, p. 3). These definitions imply exhaustive estimates of similarity among phenotypes."

no guarantee that the subset of views to be considered can be chosen objectively from the set of all possible views. In fact the subset of views to be considered cannot be chosen without reference to some theoretical concerns. The fixing of the terminology, which includes decisions about terms such as the leaf descriptors, already affects what points of view may count as character description.<sup>23</sup>

Adanson's method also assumes that each of such points of view should bear equal weight. To the extent that this latter assumption is intended to provide objectivity – to avoid arbitrariness (Adanson, 1763, p. cxxii) – the assumption is flawed, for the methodological decision to weight characters equally is itself arbitrary. Whewell's objections to Adanson are worries about generating the universe of possible characters, and so the set of individual classifications to be compared.<sup>24</sup> What guarantor is there that any set of characters and classifications will tend to converge on the natural classification?

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<sup>23</sup>This point was raised repeatedly, in various forms, in discussion of numerical taxonomy. For example here is an anonymous commenter questioning Sokal, reported in (Sokal, 1967, p. 27): "You say that different similarity coefficients will give you different relationships, and yet the numerical systematists must code his characters. But isn't that a kind of a priori weighting, when you do code the characters; and you claim to get around that." Sokal's response to this questioner was that the goal was to formulate character coding in binary terms. This response does not address the underlying issue.

<sup>24</sup>Whewell followed Candolle's text quite closely in presenting these objections. Candolle said it is clear that the first assumption "était fautive lorsqu'Adanson l'a faite, et qu'elle le sera très-long-temps, peut-être toujours..." (de Candolle, 1813, p. 71). Whewell omitted the "maybe forever" in reporting that the assumption is now and "long must be" false (Whewell, 1847a, p. 503).

In a sense, Adanson's assumption must be false forever, because there are infinitely many possible points of view that could be considered if one admits logically sound but vacuous characters. Whewell claimed that his requirement that groups support general propositions of a (broadly) scientific nature eliminates such trivial characters.

### 3.2.2 Successful Classificatory Science

Whewell attributed what success Adanson had not to his method of numerical comparison, but rather to “the dim feeling of Affinity, by which he was unconsciously guided” – that is, to the early, vague, undeveloped, unconscious application of the Fundamental Idea of natural affinity, and the additional relevant ideas in the domain of biological classification of biological function and organization (Whewell, 1847a, p. 503). Whewell’s point was that comparative methods succeed, when they do, only through the application of principled methods rooted in relevant theories for discriminating characters.

Candolle argued that the key method was the principle of *subordination of characters*, which explicitly weights characters according to biological theory. The success of Candolle’s preferred method does not derive from the fact of differential weighting of resemblances alone. Characters could be weighted by arbitrary principles. The important factor in successful botanical classificatory methodology is that the comparative subordination of characters is based on a principled scheme that includes consideration of the nature of the objects of classification. Whewell called the principled scheme *natural affinity*: the agreement of arrangements that are based on characters important to the nature of the classificatory objects. Physiological function, and the organization of physiological functions, are the crucial aspects of the nature of biological objects. What it is to be an organism, Whewell argued, is to be a system of organized parts that perform integrated functions.

“A change in the organization subservient to one set of functions may lead necessarily to a change in the organization belonging to others. We can often see this necessary connexion; and from a comparison of the forms of organized beings, - from the way in which their structure changes in passing from one class to another, we are led to the conviction that

there is some general principle which connects and graduates all such changes.” (Whewell, 1847a, p. 536).<sup>25</sup>

Whatever this general principle is, is responsible for the forms of organisms and provides the basis for natural classificatory methodology. The knowledge that some such principle exists suffices for the beginning of classificatory science, even if systematists lacked an understanding of the principles responsible for organic form. According to Whewell, we fix on the idea of natural affinity

“...as a general result of the causes which determine the forms of living things. When these causes or principles, of whatever nature they are conceived to be, vary so as to modify one part of the organization of the being, they also modify another: and thus the groups which exhibit this variation of the fundamental principles of form, are the same, whether the manifestation of the change be sought in one part or in another of the organized structure. The groups thus formed are related by Affinity; and in proportion as we find the evidence of more functions and more organs to the propriety of our groups, we are more and more satisfied that they are Natural Classes. It appears, then, that our Idea of Affinity involves the conviction of the *coincidence of natural arrangements formed on different functions*; and this, rather than the principle of the subordination of some characters to others, is the true ground of the natural method of Classification.” (Whewell, 1847a, p. 537).

The key to recognizing Natural Groups is to form multiple hypotheses, each of which groups on the basis of different functions, and check for agreement across hypotheses. Whatever is the cause of each organism’s form, the cause will operate holistically on different parts of the organism. In the early history of systematics, systematists did not know the nature of the cause. But Whewell argued that, because of the nature of organisms, whatever the causes of form are, they

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<sup>25</sup>In discussing organic form, Whewell’s use of “change”, “vary”, and “modify” was clearly not intended to mean historical transformation. In this cited passage (from p. 536) he seems to be speaking about changes in what is observed. The succeeding quote (p. 537) is consistent with Cuvier’s doctrine of conditions of existence (see pp. 69, 71). The doctrine provides a reason (a principle that explains) why each structure is as it is, but makes no reference to any historical account of how the structures came to be as they are. The principle can be said to “vary” when it results in different forms in distinct organisms – what is varying is the expression of the principle between one case and the next. Of course, some will not read Cuvier’s doctrine as providing a very satisfactory principle.



must operate in an organized way. This justifies the essential method for considering characters: agreement of classifications founded on characters essential to different organic systems.

The idea of comparing principled classifications is, in itself, independent of the particular ideas used to develop the principled classifications (Whewell, 1847a, p. 542). Whewell's concept of natural affinity does require that the particular classifications chosen develop from the ideas relevant to the target classificatory objects. Thus the use of natural affinity requires some minimal knowledge of what are the causes of the properties and relationships of classificatory objects. Classificatory science benefits from increased understanding of these causes, with the most essential step the initial recognition of the appropriate conceptual domain for the objects in question.

Whewell argued that the classification of minerals lagged behind botany because scientists had not committed to the correct ideas relevant to the nature of minerals (p. 52). Progress could be seen with respect to crystals, where scientists had only recently begun to clarify the ideas appropriate to the nature of crystals, especially crystalline symmetry. Though scientists had very little knowledge about the underlying causal processes in crystal formation, the idea of symmetry implies the coordination of changes in different parts of crystals. Just as in biology, Whewell identified a principle in mineralogy that underlies the coordination of changes in the classificatory objects considered as organized systems. This principle justifies the use of Natural Affinity. The causal processes of crystal formation are *somehow* coordinated such that symmetrical parts match each other. Natural classifications based on crystal form, macroproperties (optical properties, cleavage, and so on), and material composition must coincide to the extent that these characters relate to causal processes that act in a coordinated manner in crystal formation.

According to Whewell, the history of classificatory efforts follows a broad pattern: sporadic advances owing to blind chance; modest success owing to intuitive appeal to vague ideas about the nature of the classificatory objects; and greater success with explicit recognition of comparative

methods based on a firmer grasp of the nature of the objects. The pattern lines up with progressive development and application of the Fundamental Ideas of resemblance (seen especially in the development of terminology for describing characters) and natural affinity (in the use of comparative methods). For example, Candolle (and subsequent researchers - Mayr, 1982) emphasized Cuvier's (1817) use of functional reasoning. Whewell attributed Cuvier's successes to latent use of natural affinity, arguing that Cuvier grounded his version of subordination of characters on the consilience of different organic functions (Whewell, 1847a, p. 538). Consideration of the history of systematics led Whewell to develop his account of natural affinity. Whewell's natural affinity is a technical refinement of his concept of consilience (chapter 6.2), applied with the domain of classificatory science.

Whewell lauded Candolle's system and credited Candolle with explicit recognition of the method of agreement of functional classifications, quoting the following passage from Candolle's (1813, pp. 83-84) *Théorie Élémentaire de la Botanique*:

“... the natural classes founded on one of the great functions of the vegetable are necessarily the same as those which are founded upon the other function; and I find here a very useful criterion to ascertain whether a class is natural: namely in order to announce that it is so, it must be arrived at by the two roads which vegetable organization presents.” (Whewell, 1847a, p. 539).<sup>26</sup>

Candolle's primary division of plants following “two roads” exemplifies the use of natural affinity.

Candolle considered first the function of nutrition, and claimed that the most important characters connected with nutritive organs support a division of plants into two groups: vascular plants

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<sup>26</sup>Evidently this is Whewell's own translation of Candolle's French. There was a much edited English translation of a German translation, published as de Candolle and Sprengel (1821) that contains only a summary of this passage, on pp. 112-113. The original reads : « ...les classes vraiment naturelles, établies d'après une des grandes fonctions du végétal, sont nécessairement les mêmes que celles qui sont établies sur l'autre, et je trouve ici un critère très-utile pour reconnaître si une classe est naturelle ; c'est que, pour la proclamer telle, il faut y être arrivé par les deux voies que présente l'organisation végétale ; ... ». The two great functions are growth and reproduction.

(*végétaux Vasculaires*) and cellular plants (*végétaux Cellulaires*). Candolle then claimed that the primary division made on the basis of the function of reproduction, the *Cotylédonés* versus *Acotylédonés*, aligns perfectly with the nutritive classification: vascular plants have cotyledons, and cellular plants do not. Within the *Cotylédonés*, Candolle finds that a classification based on the position and manner of growth of the vessels (which he takes to be the most important nutritive parts) aligns with a classification based on the relative position of the cotyledons (the key sexual parts). The agreement of classifications formed on the nutritive and reproductive functions supports division of *Cotylédonés* into *Monocotylédonés* and *Dicotylédonés* (de Candolle, 1813, pp. 207-212).

### 3.2.3 Justifying Natural Affinity

In developing his concept of natural affinity, Whewell attempted to ground this comparative method by specifying the sense of “most important.” Some justification was needed to identify which characters are most important with respect to a function, and which functions are most important with respect to an organism. The requisite sense of importance is not the same as the importance for survival. Whewell sought the characters and functions by virtue of which an organism is the sort of thing that it is. Whewell discussed many failed historical attempts to define “Life”, and decided that the greatest partial successes derive from the idea of organization. What, then, is organization, in context of the organic world? Whewell endorsed Kant’s definition:

“An organized product of nature is that in which all the parts are mutually ends and means.” (Whewell, 1847a, p. 573).

Whewell reasoned further:

“If the parts of organized bodies are known to be means to certain ends, this must be known because they fulfil these ends, and produce certain effects by the operation of a certain cause or causes.” (Whewell, 1847a, p. 573).

Whewell then indicated that the search for the *cause* in question is the search for the principle of life itself. He insisted that we must not presume that there is only *one* cause. Rather, life is the totality of the operation of the causes that are mutually means and ends in organized bodies. Life is a *system of functions* (Whewell, 1847a, p. 574).

Whewell immediately recognized the objection that this definition presupposes an understanding of the idea of “Functions”. Whewell did not consider this a problematic circularity, because he claimed that definitions are not the end of reasoning; rather, the definition “*points out the course of investigation*” (Whewell, 1847a, p. 575). The idea of function will be explicated in the gradual progress of science (indeed, clarification of the concept constitutes the progress of science).

Whewell did expect that his readers would have a clearer grasp on “function” than on “life”.

Whewell explores historical attempts to describe and classify the vital functions, with candidates including respiration, circulation, digestion, and locomotion. Whewell focused his analysis of Natural Affinity on organisms’ functions because he thought that the healthful performance of these functions was the reason that the organisms exist as they do, and that the causal explanations of organic form would ultimately refer to these functions.

Following this discussion, Whewell argued that the causal relations among organic functions are strongly teleological: not only are physiological processes mutually causes and effects, they must be understood as ends and means (Whewell, 1847a, pp. 619-620) . Whewell argued at length that physiology has achieved its successes through the use of teleological causal reasoning (Whewell, 1847a, book IX, chapter 6) . He expected that the important causal relationships, processes, and ultimately the discoverable causal laws about organisms will involve teleological ends and means. The relation between Whewell’s views on teleo-functional reasoning and Design arguments is explored in the last section, below (p. 68). In chapter 5 I explore a completely different Whewellian

Fundamental Idea, *historical cause*. Introduction of historical causal reasoning to systematics would shift the framework and justification of consilience in biological systematics.

The use of consilience in the form of natural affinity presumes that there is *some* causal framework underlying the phenomena: causal processes and relations by virtue of which the entities have the forms, parts, and relationships that they have (Whewell, 1847a, p. 537). Understanding organisms as integrated systems of functions provides crucial insight into the causal framework. However, Whewell knew well that this left open the question of the initial origin of such integrated systems (Whewell, 1847a, p. 625). There remained a great deal that Whewell and other early nineteenth century workers did not know about the nature of the causal story underlying the natural classification. Nineteenth century systematists did, for the most part, expect that there is some one natural classification. The real patterns of biological life could be discovered.

Whewell's philosophy of science allowed for the application of multiple ideas in biological classificatory science – indeed he saw the availability of distinctly derived classificatory claims as a strength of systematic reasoning. Different theories might deal with different causal processes and provide crucial insight into the causal structure of the natural classification. Whewell expected classifications based on physiological systems to converge because he argued that physiology provides knowledge of the causal processes, properties, and relations by virtue of which the organism has its place in the natural classification. This expectation is grounded on (as yet, in the early nineteenth century, vague) beliefs about the nature of the causal story underlying the natural classification. Systematists also reasonably expected that comparative morphology and developmental biology would have something to do with the reasons that organisms have the forms that they have, and the biologically meaningful resemblances that they have to other organisms. Development of morphological and ontogenetic research programs could be expected to provide key insight into the natural classification.

There is a sense in which chemistry and physics also pertain to the causal story underlying the natural classification. However, certainly in the nineteenth century, knowledge of chemistry and physics could not be brought to bear as evidence in support of some hypothesized classification versus another. Nineteenth century physics and chemistry knowledge could not provide traction on questions of the form: what would have to be different about the world in order that a different natural grouping obtain? Comparative morphology, physiology, and developmental biology can potentially provide such traction.

Whewell's consilience describes the successful convergence of evidentiary claims about the world: not the reduction of distinct causes (Whewell expected that there are different kinds of cause and causal processes), nor the merging of distinct research programs. Researchers converge on the answers to certain particular questions. In the case of the classificatory sciences, a single natural classification can in principle be achieved. This natural classification is true of the world, and reflects (potentially unknown) underlying causal processes.

Whewell was, himself, pessimistic about the prospects for discovering ultimate historical causes. He was skeptical also of the prospects of discovering the reason for the existence of the natural classification with its underlying causal framework. He linked this skepticism to his views on natural theology, to which I now turn.

### 3.3: Whewell's Natural Theology

Historically and philosophically, teleo-functionalist accounts of organisms and natural theological Final Cause ascriptions are separable. For example, Owen's anatomical research program utilized means-ends reasoning to describe and explain organic forms and functions without reference to Design (Griffiths, 2007; see Lennox, 1992 for discussion and other examples,

descended from Aristotle). Each part can only be understood in terms of its relation to the system, which implies a teleological cause. A part is a *limb* by virtue of its function within the organic system. Note this concept of function is distinct from whatever it is that the organism uses the part to do: play, scratch, eat, and so on. The concept is also distinct from the reason that the part exists as it is. Owen's functional reasoning is teleological in that parts are understood by reference to goals within the context of a containing system (the organism). But Owen's reasoning did not require hypotheses about the reasons that organisms come to have the goals and goal-directed systems. Whewell was familiar with Owen's research program – indeed, Sloan (2003) has argued that the two exerted considerable influence on each other's work, with Owen advising Whewell during preparation of the *Philosophy of the Inductive Sciences*.

Another of Whewell's chief biological sources, Georges Cuvier, was also a proponent of teleo-functional reasoning. Cuvier grounded his teleo-functional reasoning in terms of necessary adaptation as a precondition for existence. Cuvier presumed that an organism whose parts failed to coordinate in service of organized functions relative to the external environment simply could not survive. Thus, we are justified in presuming that all organisms that do exist have parts and processes coordinated in service of functional ends, even if we lack knowledge of the reason that the organism came to exist as it does. Though Cuvier held that the functional coordination accords with God's Design, the idea of Design was not a necessary justification for his functionalist methodology.

Whewell's scientific sources on the use of physiology in systematics did not link teleological reasoning to natural theology. The natural theology program itself had separate argumentative and explanatory concerns versus the inductive sciences, and functioned primarily in the opposite direction: from scientific claims to arguments about the Creator (O'Flaherty, 2010). Whewell wore both natural theology and philosopher of science hats, and to a degree, his theological and teleo-functional concerns can be understood as operating within these distinct traditions separately.

Writing his Bridgewater Treatise, Whewell applied natural theological arguments to the results of the inductive sciences (Whewell, 1833). This does not imply that Whewell, when working as a philosopher of the inductive sciences, would apply natural theology to his analysis of scientific reasoning.

Whewell seems to have recognized distinct possible forms of teleological reasoning. One of Whewell's new additions to the third edition of the *History of the Inductive Sciences* concerns Transcendental Anatomy and the possible theological implications thereof:

“There is another aspect of the doctrine of Archetypal Unity of Composition of Animals, by which it points to an Intelligence from which the frame of Nature proceeds; namely this: - that the Archetype of the Animal Structure being of the nature of an *Idea*, implies a mind in which this Idea existed; and that thus Homology itself points the way to the Divine Mind. But while we acknowledge the full value of this view of theological bearing of physiology, we may venture to say that it is a view quite different from that which is described by speaking of ‘Final Causes’, and one much more difficult to present in a lucid manner to ordinary minds.” (Whewell, 1857, pp. 561-562).

Whewell accepted that the results of biological science are appropriate material for theological argument, just as the results of physical astronomy were appropriate for Whewell's *Astronomy and General Physics Considered with Reference to Natural Theology* (1833). The direction of reasoning here is from the scientific theory or facts to an inference about the Creator (that He exists; that He Created using the Idea of the vertebrate Archetype). These projects are distinct from the use of Final Causes within physiology. Whewell's *History* addresses teleological reasoning in physiological methodology at length. The use of teleological reasoning within science is what Whewell holds to be “described by speaking of ‘Final Causes’”, which is “quite different” from the view in the above quote on the theological bearing of the results of transcendental anatomy.

Whewell in fact did extend Design-end reasoning to physiological methodology. Whereas reasoning about homologous structures and the unity of type pointed toward the Divine Mind, the existence of adaptive structures pointed toward purposefulness in the organisms themselves. The



crucial point is that Whewell claimed the causal story behind natural classification is a story of Design, and extended teleological reasoning to the reason that organisms exist as they do.

“...it has always been dwelt upon as a favourite contemplation, and followed as a certain guide, by the best anatomists; ... that in every organized structure, plant or animal, each intelligible part has its allotted office: - each organ is designed for its appropriate function:- that nature, in these cases, produces nothing in vain: that, in short, each portion of the whole arrangement has its *final cause*: an end to which it is adapted, and in this end, the reason that it is where and what it is.” (Whewell, 1847a, p. 80).

Whewell thought that the mutual interdependence of functions towards ends not only describes the causal structure of the organism as it stands, but explains the reason why the organism came to be as it is with the natural relationships that it has. Cuvier’s reasoning about ends was compatible with a kind of agnosticism about origins: however and why-ever organisms come to be, they can only have come about as functionally organized systems, else they could not survive. Whewell explicitly argued that the functional organization results from intention.

Citing Owen (1834), Whewell described the “contrivances” by which newborn kangaroos are able to suckle successfully, and concluded:

“The parts of this apparatus cannot have produced one another; one part is in the mother; another part in the young one: without their harmony they could not be effective; but nothing except design can operate to make them harmonious. They are *intended* to work together; and we cannot resist the conviction of this intention when the facts come before us. Perhaps there may hereafter be physiologists who, tracing the gradual developement [evolution] of the parts of which we have spoken, and the analogies which connect them with the structures of other animals, may think that this developement, these analogies, account for the conformation we have described; and may hence think lightly of the explanation derived from the reference to Final Causes. Yet surely it is clear, on a calm consideration of the subject, that the latter explanation is not disturbed by the former; and that the observer’s first impression, that this is “an irrefragable evidence of creative foresight,” can never be obliterated; however much it may be obscured in the minds of those who confuse this view by mixing it with others which are utterly heterogeneous to it, and therefore cannot be contradictory.” (Whewell, 1847a, p. 625).

Whewell claimed that cases such as the kangaroo impress us with the conviction that God has designed organisms as they are. The appeal to Design provides an explanation of the origin of the

system that enables kangaroo infant feeding. This explanation is compatible with explanations that derive from physiology and comparative morphology without direct reference to Design. The origin of the system, achieved through whatever immediate causes, is ultimately explained through the Creator's foresight.

Arguments about the Design of biological systems fill a justificatory role in Whewell's system. Physiological ends-based reasoning works to explain the existence of natural systems and natural relationships, on Whewell's view, because God designed the systems with the ends discoverable through physiological reasoning. Though Whewell's theological justification is particularly visible here, it should be remembered that Whewell appeals to God's creative act to justify all scientific reasoning (see p. 45).

Whewell's approach to systematics ultimately appealed to divine final causes to explain the reason that the natural classification exists. However, his concept of natural affinity was justified by reasoning about more immediate causes of biological form, namely, the functional integration of biological systems. Chapters 5 and 6 concern the application of consilience in systematics without the ultimate appeal to Design, and with an alternative causal framework: historical causation.

At the moment, I turn to the prospects of the Millian framework for induction in systematics in light of Whewell's critiques.

#### 4.0 NETWORK ASSUMPTIONS AND THE RELEVANCE PROBLEM

In this chapter I begin to attack the nature and status of systematics qua *historical* science. My first concern is to critique some framing assumptions that pervaded twentieth century philosophical accounts of the historical sciences. I show that challenges to the status of the historical sciences rest on the faulty assumptions that I criticize. I also show how the assumptions underlay twentieth century debates on explanation in science in general.

I first (section 1) identify the assumptions, which descend from Mill's philosophy of science, and show their role in the covering law model and subsequent models of explanation. I then (section 2) relate Whewell's criticism of Mill's methods, and Whewell's philosophy of science, to the relevance problem that pervaded twentieth century explanation debates. The assumptions lead to descriptively inaccurate philosophy of science. The following chapters presents my alternative account, developed from Whewell's analysis of the historical sciences (chapter 5) and (relatedly) an account of inference to the best explanation (chapter 6).

## 4.1. THE NETWORK ASSUMPTIONS AND MODELS OF EXPLANATION

### 4.1.1 The Network Assumptions and the Covering Law Model

Unlike the Humean, Mill held that causal necessity exists and figures in scientific laws; but like the Humean, Mill thought of causation as a link between discrete events. Milleian laws express regularities that obtain among events. From Hume comes the atomicity of entities as potential causal interactors, between which an independent linking relation (the “necessary connexion” between cause and effect) is to be sought. From Mill comes the view that scientific laws describe the causal links by referring to event types. Particular causal transactions are instances of the relations of causal necessity as described by Milleian laws. The overall picture is that science investigates a network of event transactions. Scientific hypotheses reconstruct parts of the network by identifying nodes and connections between nodes. Nodes are spatiotemporal particulars understood as tokens of types. Links between nodes are causal transactions described by Milleian causal laws.<sup>27</sup>

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<sup>27</sup>I have introduced the network framework using only events as nodes. My arguments will hold for any causal ontology that admits of only spatiotemporal particulars understood as tokens of types. What matters for the arguments is that causal laws express relations between types, and latch onto particulars in the network by virtue of those particulars’ token status.

Salmon’s causal networks are constructed of causal processes; the issue (as Hitchcock, 1995 noted) for what follows is whether and how the process ontology handles *properties*. Hitchcock’s criticism, discussed below, is that Salmon’s proposed explanations cite properties only as referents to the processes that figure in causal transactions, just as causally relevant (theory) properties serve as referents to the events cited on the events-only network model I have drawn from Mill. The properties delineate types, relative to which the cited processes are spatiotemporal particular tokens.

Helen Steward (1997, pp. 214-215) noted that Mill explicitly allows token states as causal relata citing Mill’s discussion of a man who dies after eating a particular dish.. Mill claimed that all of the antecedents, including both those typically called conditions and causes, are part of the cause, “philosophically speaking”: “What in the case we have supposed disguises the incorrectness of the expression is this: that the various conditions, except the single one of eating the food, were not

These parts are wholly atomic, so that any part or parts of the network can be described without reference to any other part of the network. The view that I am describing characterizes science in general. Presently I discuss the network assumptions in terms of explanation as a scientific aim, and below (section 2.3) I discuss the influence of the model in understanding historical scientific hypotheses and methodology.

Twentieth century philosophical discussion of explanation, and the status of the historical versus physical sciences, was rooted in debate on the adequacy and scope of the covering law model. The classic locus is Hempel 1965, though the covering law model has roots in Mill and further back (Hempel, 1965, p. 251 fn 257) all the way to Aristotle (Schaffner, 1993, p. 265). According to the covering law model of explanation, the explanans describes a set of conditions and a law or laws. On the deductive-nomological version of the model, the components of the explanans together logically entail the explanandum. On the inductive-statistical version, the relation is probabilistic: the explanans provides grounds to expect the explanandum because the explanans renders the explanandum highly probable.

Covering law explanations fit neatly with Mill's view of causal laws and the network reconstruction view that I have described above. Both the explanandum and the conditions cited as

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*events* (that is, instantaneous changes or successions of instantaneous changes) but *states* possessing more or less of permanency..." (Mill, 1843a, p. 399).

Indeed Mill rather casually (perhaps carelessly) wrote of events, states, things, and facts as causal relata. If Mill's view of the causal relation can obtain among these entities, they must be understood as tokens relative to types that are described in Milleian causal laws. The view that is typically called Milleian parity is that, for the purpose of causal calculation, events, states, objects, and facts as causes can be treated as ontologically on par. Causes and conditions add up like vector sums; causal links can be modeled as force vectors between nodes. On this view, descriptions of causal interactions can be translated into descriptions of event transactions. Facts are simply descriptions of events, things only partake in causal transactions via involvement in events, and states are chains of successive event atoms. Steward discussed the example, "the log caused the flood", which might be converted to "the event(s) involving the log [the log smashing the dam, perhaps] caused the event(s) of the flood."

part of the explanans are typically conceived as token spatiotemporal particulars (usually, events) that are instances of types (Hempel, 1965, pp. 231-233).<sup>28</sup> The causal laws cited in the explanans describe relations that will obtain (necessarily or probabilistically) between instances of these types. A covering law explanation offers a series of events by which the state of affairs ("the occurrence of a particular instance of a given kind of event" - Hempel, 1965, p. 423) described in the explanandum comes to pass, with causal laws guaranteeing the succession of each event in the series. Covering law explanations are positive hypotheses about what are the nodes and links in the causal network in which the phenomenon of interest is embedded.

#### 4.1.2 The Network Assumptions and Historical Explanation

Hempel's formulation of the covering law model included the *symmetry thesis* that predictions are simply explanations made at a time prior to the occurrence of the event cited in the explanandum. On this view predictions also reconstruct portions of the network of spatiotemporal particulars, portions which have not yet transpired. Covering law explanations are of the same form, whether they address phenomena that are presently accessible, predicted to occur in the future, or hypothesized to have occurred in the distant past. The fact that long past events are typically more difficult to access presents an epistemic problem for the historical sciences. On Hempel's view, the scarcity of (if not total lack of) laws in the historical sciences presents another epistemic problem for these sciences. Hempel (1942; reprinted in Hempel, 1965) proposed that historical scientists offer

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<sup>28</sup>More strictly, both the explanandum and explanans are sentences that describe the spatiotemporal particular tokens and laws. I do not address the issue of whether subsumption of laws by more general laws fits the very same covering law model so that the explanandum is a regularity rather than an event, as Hempel (1965, pp. 247, 273) proposed (but worried about - Schaffner, 1993, p. 266 and fn 210).

explanation sketches that match the covering law form but utilize only probabilistic and often imprecise causal regularities that are best viewed as promissory notes toward proper causal laws.

Some philosophers argued that in practice, historiographers construct *historical narratives* as a distinct form of explanation that does not involve causal laws. A historical narrative proposes a series of connected events in the light of which the event to be explained falls into place – or in Hull’s (1975) terms, is integrated “into an organized whole.” Proponents of the covering law model object that the “falling into place” must be cashed out as a deductive (or inductive statistical approximation of a deductive) relation between the antecedent events and the explananda, and that this relation must be explicable as a general law. On this argument, historical narrative explanations are simply covering law explanations with implicit general laws (Ruse 1971; 1973). Hempel’s worry recurs, as the implicit general laws are frequently sketchy and imprecise.

In response, Thomas Goudge (1961) pointed to the fact that historical narratives apply to particular, non-repeatable phenomena and argues that unique phenomena cannot be the subject of general laws. Michael Ruse replied by attempting to unpack Goudge’s use of the term “unique”, which Goudge had indicated applied only to a limited set of explananda, for example the extinction of a particular lineage or the colonization of land by vertebrates (Goudge, 1961, p. 70). Ruse pointed out that every event is “unique” and non-repeatable in at least the limited sense that each event bears unique spatiotemporal coordinates. The fact that an event is unique in this sense does not mean that a general law cannot be applied to the event. Ruse offered the example of a particular instance of a pendulum swing, which is the subject of general laws about the motion of pendulums. Ruse considered also the idea that “unique” indicates a particular combination of conditions, but again argued that this sense of unique does not bar the application of a general law, for the particular combination could recur somewhere.

Goudge's careful phrasing on the issue of uniqueness is worth quoting:

“What we seek to formulate is a temporal sequence of conditions which, taken as a whole, constitutes a unique sufficient condition of that event. This sequence will likewise never recur, though various elements of it may. When, therefore, we affirm 'E because s', under the above circumstances, we are not committed to the empirical generalization (or law) 'Whenever s, then E'. What we are committed to, of course, is the logical principle 'If s, then E', for its acceptance is required in order to argue 'E because s'. But the logical principle does not function as a premiss in an argument; the affirmation, 'E because s', is not deducible from it (Cf. Ryle, 1950). Both s and E are concrete, individual phenomena between which an individual relation holds.” (Goudge, 1961, pp. 77-78).

At issue here is an assumption about causal ontology. For Ruse, causation is a matter of general laws that describe classes of transactions between events. Events are spatiotemporal particulars in themselves but insofar as they participate in causal transactions, they are understood as tokens. The causal relation obtains between token events. For Goudge, an *individual* causal relation holds between antecedent and consequent, and in recognizing this individual relation we are not committed to any general causal law (see Cartwright, 1994; Harré & Madden, 1975 for theories of causation under which attempts to form causal laws are at least sometimes unnecessary, misleading, and perhaps incoherent).

In terms of causal explanation, Ruse argued: if the “falling into place” relation is not cashed out via general laws of deductive (or inductive statistical) form, it must appeal to some other rule of inference. Ruse claimed the only available alternative rules of inference that might join unique, non-repeatable phenomena are horrifyingly esoteric, taking the form “on this particular occasion, we are justified to infer this particular q from this particular p” (Ruse, 1973, p. 71). The argument turns on this point: if not in terms of causal laws, how do historical narratives represent the connections between events such that the connections render the explananda expected?

Ruse's argument presupposes that explanations must utilize rules of inference that apply to entities understood in terms of a network of causal interactions between tokens. Each token event that figures in a historical narrative explanation is unique and non-repeatable if for no other



reason than its unique space-time stamp. However, Ruse assumed that explanations must latch onto these events as instances of types. Proper, upstanding rules of inference utilize types; in the case of causal explanation, causal laws (and any weaker, law-like generalizations) describe relations between types that are grouped together via causally relevant properties. Causal laws of this form can only be applied to unique events if those events are understood as token instances of types delineated by a scientific theory. Atomic events are described as tokens of types, and causal links are described by rules of inference that refer to these types.

Goudge anticipated something like Ruse's argument that historical narrative explanations are in fact implicit covering law explanations. Goudge's response follows from his descriptive claim that science does not match what I have identified as Mill's network assumptions (Goudge, 1961, pp. 74-78).<sup>29</sup> Goudge argued that historical narrative explanations should not be modeled as reporting events in causal chains with causal laws describing links between atomic events. Such a model obscures the role of scientific background knowledge in the historical explanation. Goudge's point is that the chain model itself suggests that atomic events and their descriptions cry out for causal laws to secure connections. We should instead understand each component of the explanation as interconnecting with the whole corpus of the scientific context, in a way that cannot be captured by atomic nodes and chains. Goudge's proposed historical narrative explanations do not refer to event types identified via causal laws. Scientific background knowledge, including inductively grounded

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<sup>29</sup>Goudge considered the deductive-nomological model and the atomic chain of events models separately, without explicitly spelling out the connections between them (which I have sketched here). The discussion is framed in terms of whether the historical sciences conform to the same model of explanation as do the other, regular sciences (of which physics is the prime exemplar). The covering law model is more or less presumed to be the correct model of explanation for the regular sciences, though Goudge alluded to reservations about the appropriateness of the atomic chain model in these sciences as well, citing (Hanson, 1955). Ruse's critique of Goudge also framed the issue as whether the historical sciences differ from the other sciences in terms of explanation. As I see it, the key arguments in the exchange hinge on the atomic model.

causal knowledge, provides explanatory force without requiring atomically delimited rules of inference. Ruse's "what rule?" objection can be avoided by avoiding the network assumptions that underwrite it.

Goudge's point has not been sufficiently appreciated with respect to the historical sciences. The covering law model has lost much of its hegemony in philosophy of science. Yet many philosophers have retained the network assumptions, particularly in descriptions of the historical sciences.

For example, the assumptions underlie Robert O'Hara's description of the task of phylogenetic inference in generating historical explanations. O'Hara identified two key concepts, *chronicle* and *history*: "a *chronicle* is a description of a series of events, arranged in chronological order but *not* accompanied by any causal statements, explanations, or interpretations" (O'Hara, 1988, p. 144). Event *p* happened, and then event *q* happened, and so on. "A *history*, in contrast to a chronicle, contains statements about causal connections, explanations, or interpretations" (O'Hara, 1988, p. 144). O'Hara argued that the task of systematics is to hypothesize segments of the *ideal chronicle* (the complete series of events in the history of life on earth).<sup>30</sup>

On O'Hara's view, explanation requests can sometimes be met by filling in events to provide a more complete chronicle. Filling in chronicle segments may answer "how-possible" explanation requests (Dray, 1957), and also may answer causal explanation requests provided that the information-seeker is willing to graft causal content onto the chronicle using background knowledge. Causal explanation requests are more explicitly answered by providing information about the causal connections obtaining of the events in the chronicle. Evolutionary biology provides the causal

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<sup>30</sup>I focus on O'Hara's use of the chronicle-narrative distinction in handling causal content. O'Hara also uses the distinction to explore issues of interpretation, perspective, and objectivity in the historical sciences (see especially O'Hara, 1992), and in discussing these issues appears to shift the essence of the chronicle/narrative distinction.

content that is needed to construct histories that provide the latter kind of explanation. This can be done by citing causal laws of which the connections are instances, in keeping with the covering law model; or, if Ruse's challenge can be met, by providing causal content that somehow makes the chronicle events "fall into place" without reference to causal laws. Because chronicles lack causal content, they cannot in themselves provide positive causal explanations. They fulfill explanation requests either by countering objections that an event or series of events was impossible, or by providing information that is explanatory only in conjunction with background theoretical knowledge that is external to the chronicle itself.

This framework retains the network assumptions, though avoiding the claim that all explanations are reducible to (sketches of) covering law explanations. Explanations that cite only the chronicle provide positive hypotheses of chains of events in the network, whether this is done to counter a "how-possibly" objection or fill in key events to which the questioner will apply her own causal knowledge. Histories apply causal knowledge to the "links" between atomic events described by chronicle segments, so forming positive hypotheses about event chains comprising both atomic events and their causal connections. Explanations characterize the connections in terms of inference rules, which might take the form of Millian causal laws or some other form. Ruse's challenge remains.

I argued above that the network assumptions themselves set up the challenge. If historical narratives do not cite Millian-style covering laws (or approximations thereof), how can they explain? The problem could be avoided by dropping the network assumptions, but some philosophers and systematists have not taken this route. I turn now to problems with the network assumptions that give us reason to abandon the model in philosophy of science in general and philosophy of historical science in particular. Abandoning the network assumptions would open a space for proponents of

historical narrative and other forms of explanation to offer causal explanations without having to provide rules of inference that meet the requirements of Millian-style causal laws.

## 4.2. THE NETWORK ASSUMPTIONS AND THE RELEVANCE PROBLEM

### 4.2.1 The 'Hard Part' Objection

Explanations that offer network reconstructions are vulnerable to the objection that descriptions of events and event transactions may fail to explain, if the descriptions fail to capture properties that are causally relevant relative to the explanation request. The atomicity of causal claims generates the problem, which is typically called the relevance problem. The relevance problem recurred throughout twentieth century debates on explanation.

For example, Salmon (1984) criticized the covering-law model for its failure to meet the relevance objection. Salmon proposed that causal explanations describe portions of the *causal nexus* in which the explanandum is embedded. The causal nexus consists of mark-transmitting causal processes. Causal interactions occur when causal processes intersect in such a way as to support certain counterfactuals. A good causal explanation supports such counterfactuals: had the cited portions of the causal nexus not obtained, or had any cited causal law not obtained, there would have been a difference in the outcome. Salmon offered the case of a man, Mr. Jones, who takes birth control pills and avoids becoming pregnant. The covering law explanation of the outcome cites the event of the pill-taking and a law that taking birth control pills prevents pregnancy. The example offers a good causal law and meets the form of the covering law model, yet runs afoul of the relevance objection. The event of Mr. Jones taking the pill was irrelevant to the outcome. Salmon

argued that his own model handles the case properly, because the event of Mr. Jones' pill-taking made no difference to the outcome, and the example explanation therefore fails to meet the model requirements of a good explanation.

Salmon's model of explanation makes the familiar network assumptions. Causal explanation requests are handled by describing the causal nexus, a network of processes that are latched onto as tokens of types that figure in descriptions of causal regularities. The regularities function in the explanation to secure "links" between the token nodes cited in the description of the nexus (network).

Salmon's model remained vulnerable to the relevance problem. It is possible to construct explanations that meet the formal requirements of Salmon's model, referring to the operative causal processes and supporting the appropriate counterfactuals, yet which fail to secure explanatory relevance.

Hitchcock offered an example (1995, p. 310): a pool player chucks her stick, and then strikes the cue ball. Her strike both propels the cue ball and leaves a blue chalk mark on it. The cue ball then strikes the eight ball, and the eight ball sinks into the corner pocket. Hitchcock constructed an explanation of the sinking of the eight ball that cites the sections of the causal nexus that precede the explanandum, including the causal interaction between the stick and the cue ball, the subsequent motion of the blue-marked ball, and the causal interaction between the blue-marked ball and the eight ball. The explanation also cites a law that systems that contain blue chalk marks exhibit conservation of linear momentum. The causal links in the network are described by this law, so that the explanation cites a series of events and processes and refers their causal connections to the law about systems with blue-marks (together with some unpacking of "linear momentum"). The example explanation meets Salmon's requirements of a good explanation, yet Hitchcock points out that intuitively, the citation of the law involving the blue chalk mark is faulty. The blue mark is

irrelevant to the sinking of the eight ball, even though the counterfactuals hold up and the portions of the causal nexus to which the law refers do make a statistically relevant difference in the outcome. Had the cited events and processes – the player’s striking of the cue ball, and the process of the blue-marked ball rolling toward the eight ball – not occurred, the eight ball would not have sunk. Had the law about systems with blue marks failed to obtain, the eight ball may not have sunk (depending on the way in which such systems diverge from the cited law).

Hitchcock (1995) demonstrated the vulnerability and argued that such failures can occur because explanatory relevance comes from *properties*. Nothing in the covering-law model, and nothing in Salmon’s account, ensures that our explanations refer to explanatorily relevant properties. This is a general feature of explanations that cite segments of an atomic network of causal interactors. Properties can only provide explanatory relevance via reference to the events and processes as tokens that figure in causal transactions. Hitchcock’s objection is that this is just wrong: properties can be explanatorily relevant or not (and perhaps to degrees), and this relevance may not line up with the properties’ role as referents to events or processes.

Properties may serve two distinct roles in causal explanations: providing explanatory relevance, and securing reference to the entities that are relata of causal transactions. Models of explanation cannot assume that the same properties automatically fulfill both roles. Properties that are taken to be relevant ways of referring to types of causal transactions may be irrelevant to particular causal explanations. This occurs when the theory relative to which the cited properties are causally relevant is not appropriate to the explanation request, that is, when the theoretical context that accompanies the proffered causal laws and concepts is not appropriate to the explanatory task. Network reconstruction explanations are vulnerable to the relevance problem because they cite entities and causal regularities by whatever properties delineate them as atomistic tokens and type-regularities, regardless of the context required by the explanation request.

The relevance problem crops up as a result of failure to deal with the “hard part” objection that Whewell launched against Mill (chapter 3). Mill’s methods presume that the scientist has identified atomic events, already categorized as tokens of type A, B, C, and so on. The scientist then tests combinations of these event types to determine whether Millian causal laws obtain such that event tokens of these types will be linked via causal transactions. The problem is that this scheme presumes that the properties that are used to delineate A, B, C (i.e. that serve the reference role) are also the properties that are going to be relevant to individual explanation requests. Mill’s causal laws can only serve to explain if they use properties that both secure reference to the appropriate class of theoretical entities and are those very properties that are relevant to the explanation request at hand. To satisfy this requirement amounts to completing the hard part, the grouping of entities in the world in just the way required for both roles.

Whewell’s philosophy of science suggests a way to avoid running afoul of the relevance problem. Whewell avoids the network assumptions, instead focusing on conceptual development and the ways in which knowledge appropriate to different scientific domains is interconnected. Scientists grapple with the “hard part” directly, forming a concept that identifies or refers to the properties that are causally relevant with respect to target phenomena. Addressing the relevance problem just is what scientists do, and it is not done in terms of atomistic nodes and links. Goudge (1961, p. 75) also advised avoiding the network assumptions and emphasized the interconnected nature of scientific knowledge and explanations. Dropping the network assumptions can enable philosophers of historical science to avoid the “what rule?” argument launched by Ruse (1973).

Dropping the network assumptions also puts us in a position to better understand the relevance objection. Hitchcock (1995, p. 310) noted that a critic may claim that the fault lies in the supposed law itself rather than in the explanation’s citation of the law. There is nothing formally wrong with the law about conservation of linear momentum in objects that have blue chalk marks,

and (we are confident) the law does hold in the world. The objection seems to be that no actual scientific theories would ever formulate laws in the domain of mechanics on the basis of such a formally acceptable but intuitively inane causally relevant property as “has a blue chalk mark”. However, this fact about scientific practice is not an objection to the possibility of such explanatorily irrelevant laws being formulated and used. Such cases rarely arise in science precisely because scientists address the relevance objection head-on, in what Whewell identifies as the hard part of science.

#### 4.2.2 The Conceptual Structure of Science

The twentieth century philosophical debates on explanation took the form of one author proposing a model of scientific explanation, only to be confronted with a counter-example such as the Jones pregnancy case or the blue chalk case. The counter-example fits the proposed model of explanation, yet it is claimed that an appeal to intuition reveals the example explanation to be faulty. The model is altered or replaced; but soon a new counter-example is offered, and the cycle proceeds. The above discussion, like the explanation debates themselves, appealed to “intuitions” about relevance. We can achieve a better descriptive account of science by examining these appeals to inanity.

The proffered counter-examples exploit a mismatch between our conceptual structure of the world and a proposed grouping of entities that is logically permissible, about which a Millian causal law can be formed. Our conceptual structure of the world is reflected in our intuitions about what type of information is relevant to the explanation request, so that the explanatory “gap” described above arises in these problematic examples. And our conceptual structure of the world in turn reflects our current science. On Whewell’s view, science both shapes and is shaped by our concepts. Science simply does not proceed by casting about for possible connections between entities in the



world that can be grouped in whatever ways are logically permissible. In turn our intuitions about what groupings are inane are shaped by the structure of the world presented by current science. In the philosophical literature on explanation, the network assumptions enable the gap that underlies the appeal to intuition. Philosophers are able to imagine logically possible groupings that do not match the conceptual structure of the world that is shared, more or less closely, by current science and readers' intuitions.<sup>31</sup> The logically possible but conceptually faulty groupings are dubbed nodes in the causal network, a procedure that is possible because of the presumed atomic independence of the network elements: any grouping can be plugged in as a node, regardless of the corpus of scientific background knowledge.

Whewell's philosophy of science does not allow such moves because in Whewell's view, causal laws develop from the interplay of empirical experience with the gradually advancing conceptual structure of science. Scientists do not cast about testing for causal laws among all logically possible groupings, but rather formulate proposed laws drawing on a whole corpus of interconnected background causal knowledge, which may be reflected in intuition. Nothing in the formation or refinement of the concepts of mechanics is likely to result in serious consideration of the property, "has a blue chalk mark". Indeed there is no current scientific discipline whose concepts seem likely to lead to any theoretical or explanatory role for this property. In practice, scientists avoid the relevance objection because proposed groupings and laws are not formed in isolation from the conceptual structure of science that (more or less) matches our intuitions.

Note that my claim here is only that science is (more or less) successful in meeting the relevance problem, not that science is successful in capturing the actual causal structure in the world.

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<sup>31</sup>Note that the examples run afoul of relatively basic science: the concepts of basic mechanics just don't match grouping on the basis of colored chalk. Readers' intuitions are not likely to be aroused by conceptual mismatch when the relevant scientific concepts require advanced technical training to grasp.

Along similar lines as the pessimistic induction, one might argue that past scientific theories have grouped entities in the world via properties that turn out to be utterly inane, for example “perfect” heavenly bodies that exhibit perfect circular motion. However, such a grouping avoids the relevance problem if, in its proper historical context, the cited property secures explanatory relevance; that is, if historical contemporaries accept explanations that cite such properties. And in its proper historical context, there is nothing intuitively inane about the grouping. There is no current scientific theory that utilizes the grouping, “objects marked by blue chalk”, and no evident reason that contemporary scientists would propose such a grouping.

Another way to run afoul of the relevance objection is to use properties from actual, upstanding scientific theories that are not appropriate to the explanation request at hand. For example, we might imagine that Galileo drops a Crucifère plant off the Tower of Pisa at time  $t(1)$ . The Crucifère falls and lands at time  $t(2)$ . We ask, why did the Crucifère land at time  $t(2)$ ? Galileo offers an explanation that cites the initial dropping event, the height of the Tower, all relevant atmospheric conditions, and (among other laws) a law that asserts that the distance traveled by objects that photosynthesize is proportional to the square of the time of fall. As in the blue chalk case, the cited causal processes and law are counterfactually necessary for the explanandum, yet the appeal to the property of being a photosynthesizer is irrelevant. The property of being a photosynthesizer is causally relevant only within a scientific domain that is irrelevant for the explanation request at hand.

In chapter 3 I argued, following Whewell, that Kepler would not have been successful in discovering his laws of planetary motion had he grouped phenomena using properties that physiological theories use to group entities in making causal claims. Cases such as the imagined Galileo example do not arise in practice because, while the law about falling objects that photosynthesize is true, it is at the least wildly unlikely that Galileo would have discovered this law

without first formulating (and perceiving the superiority of) a law about the motion of falling bodies in general. In Whewell's philosophy of science, because laws are discovered via progressive refinement of concepts that are fundamentally bound up with our understanding of the domain of inquiry, it is simply impossible to formulate a law without first achieving some understanding of the conceptual groupings relevant to the domain of the law.

Whewell's philosophy of science is a useful corrective to the neat, atomic picture of scientific methodology presented by Mill and perpetuated in the widespread network assumptions. In practice we find that the way in which scientists delineate and group entities prior to causal testing already indicates what causal theories and causal laws apply to the entities. Scientists do not hypothesize chronicle segments absent causal consideration. Instead, successful scientists find causally relevant properties by which to group the entities in such a way that, together with the whole corpus of the scientific domain, the entities share properties that are likely to be explanatorily relevant with respect to the inquiries appropriate to that domain. Causal content is already considered as the groupings of entities are formulated, in such a way that the causal content cannot be conceptualized solely as independent, atomic links between set entity types. Rather than considering properties simply as means of securing reference to groups of causally efficacious events or processes, a philosophy of science should capture scientists' continuous concern to identify explanatorily relevant properties. When different theoretical domains are relevant to an explanation request, scientists must reconcile different answers to the "hard part". I address successful reconciliation – Whewell's consilience – in chapter 6.

#### 4.2.3 Event Chains and Historical Hypotheses

In the historical sciences in particular, scientists do not graft causal content onto preformed chronicle segments to form hypothesized narratives. Most of the time, when historical scientists consider spatiotemporal particulars as tokens of types, the type groupings do not pertain to causal transactions between identified events in a historical chronicle. Rather, type groupings in historical science relate either to theories about restricting what possibly could have occurred, or to methodological theories about information-preserving or information-destroying processes that affect the evaluation of traces as evidence. I discuss the former kind of grouping in chapter 5, where I argue that most systematic hypotheses should be viewed as hypothesized facts about patterns that restrict the possible set of past events. Causal theories contribute to the evaluation of (observations of) spatiotemporal particulars as evidence for or against patterns. This evaluation does not consider the spatiotemporal particulars as events to be linked in historical chains.

In the latter form of grouping, historical scientists consider an observation of trace evidence in the present as the most recent term in a chain of events connecting to some past event of interest. For example, historical scientists utilized radiocarbon dating to estimate the age of human remains found in the Alps and dubbed “Ötzi”, “the Alpine Iceman”. This method posits the existence of a chain of events that includes individual instances of isotopic decay, linked in a continuous series from the present observation event to the past event of Ötzi’s death. The hypothesized individual isotopic decay events are tokens relative to the type of event addressed by the causal theory underpinning the radiocarbon dating method. This theory may include Milleian causal laws that describe transactions between events of this type. In keeping with the network reconstruction model, historical scientists hypothesize a positive chain of event tokens linked by causal transactions that are theorized to hold of the event type.

However, this hypothesized chain of events is not itself the item of interest. The end result of the method is a range of possible ages. Typically it is the range that is used in subsequent scientific work, rather than a (statistically supported) positive hypothesis of a particular timestamp. Historical scientists are interested in a fact about the event of Ötzi the Iceman's death (that it occurred between 3300 and 3500 years ago): this fact may figure in historical hypotheses about human migration patterns, diet, and so on. The reconstruction of events of isotopic decay does not itself figure in historical explanation except as background material supporting the facts that are the direct material of historical theories and explanations.

Tucker (2004) has emphasized this type of event-chain reconstruction between evidence (present observations) and past particular events as the central practice of historical scientists. In Tucker's view, the history of historiography shows that historical scientists have been primarily occupied in establishing methodological tools (with the requisite causal theoretical backing) to reconstruct such event chains. The tools apply to event types described by theories of linguistic transmission and evolution, fossilization, isotopic decay, and so on. What is reconstructed is not the target historical network of events and event connections in itself. Tucker argued that the covering law debates were off-target because they missed this fact about historical scientific methodology. He suggested that the problem may have stemmed from analysis of narrative event chains presented in history texts, an end product of historical methodology. He argued that the historical narratives presented in popular texts are not representative of the actual practice of the historical sciences. Rather, these are constructed secondarily as heuristic (or otherwise practical) devices.

Tucker is correct that some portions of historical scientific methodology take the form of event-chain reconstruction in order to establish links between present evidence and past events.

However, this model is extremely incomplete with respect to systematics.<sup>32</sup> With respect to the historical sciences that deal with human history, particular events are often of interest. Historical scientists investigate the event of the assassination of Archduke Ferdinand in itself: the precise time, location, and other aspects of this event qua spatiotemporal particular are direct targets of investigation. In contrast, the majority of systematic hypotheses are not about individual particular events. Gould's examples – the extinction of a particular lineage or the evolution of land vertebrates – are quite dissimilar from typical systematic hypotheses. The example explananda are easily visualized as particular events, of interest as particulars. More typical systematic questions target relationships that result from processes that are extended in space and time.

Moreover, what systematists use to formulate and test hypotheses are facts, just as scientists extract facts in the Iceman case rather than further reconstruct particular events and event transactions. For example, an individual fossil may be of interest to a systematist because of facts about the specimen's estimated age, its geographic location, morphological features, and so on, that bear on hypotheses of relationship. The fossilized organism is not itself, as an individual, represented in any systematic hypothesis, and systematists have no interest in the events surrounding the life, death, and subsequent fossilization of the individual (except and only to the extent that these events must be investigated in order to serve as evidence for facts of interest).

Tucker's analysis is particularly incomplete as regards systematics because morphological information, which has historically been at the center of systematics work, is not analyzed in terms of event chain reconstruction. As Wagner (1989) has pointed out, evolutionary biologists lack an

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<sup>32</sup>Tucker's historical and philosophical analyses focus on the human-centric historical sciences, but are intended to apply to evolutionary biology as well. Tucker (2004, p. 20) claimed that evolutionary biology adopted the theories and methods developed as the core of historical scientific methodology in the nineteenth century by biblical criticism, classical philology, and comparative linguistics.

account of the “copying” relation by virtue of which homologous morphology could be traced from descendent to ancestor (see also Griffiths 2007 and Brigandt 2002). Such an account would be required in order to define the causal connections in a historical event chain characterized in terms of morphology. This has not deterred systematists from using morphological data, because systematists do not attempt to trace morphology through causal event chains. Rather, morphological information is considered alongside contributions from other causal theoretic domains in reasoning about patterns, as will be shown in chapters 5 and 6.

Very many modern systematic hypotheses take the form of phylogenetic trees. The most conceptually fundamental modern tree diagrams are cladograms that present hypotheses of relative degree of relationship among biological entities (N. Eldredge & Cracraft, 1980, pp. 50-51, 211-215; Wiley & Lieberman, 2011, pp. 92-94). When considering systematic methodology and inference, it is not helpful to view cladograms as hypotheses about sets of historical events. Event-thinking is a heuristic, to be imposed after phylogenetic inference has produced a well-supported hypothesis about biological relationships. For one thing, the dichotomous form of phylogenetic diagrams does not match the fine-grain events that we think likely occurred in the past of the depicted groups of entities. Most processes of speciation do not result in dichotomous splits between groups of organisms, when the processes are viewed with sufficiently fine-grain. The methods used to generate tree diagrams do not assume that events would proceed in the fashion depicted on the diagrams. Indeed, those methods that do proffer particular hypothetical event scenarios about instances of allele inheritance presume that individual allele inheritance scenarios need not match the phylogenetic trees for organisms and populations of organisms. Such event chain scenarios exemplify Tucker’s (2004) point that event chain reconstruction often informs the connection between evidence and hypothesis without itself addressing the target of explanation.

In this chapter I showed that worries about the status of historical science can be side-stepped by avoiding problematic assumptions descended from Mill. Having dispensed with the view that historical explanations are narratives of event chains, we are left with the questions, what are historical hypotheses? In the case of systematics, how do these explain (chapter 5), and how do scientists make inferences from evidence to historical hypothesis (chapter 6)?



## 5.0 HISTORICAL HYPOTHESES

In this chapter I develop a framework for understanding historical science following Whewell's philosophy of historical science (section 5.1), exemplified in Whewell's own foray into historical architecture (5.1.2). I develop the concept of causal dependence from Mill's philosophy, differentiating historical causal dependence from direct property dependence. My analysis provides a better understanding of the role of causal theory in historical science than the picture I criticized in chapter 4.

I then show how biological systematics became historicized (section 5.2). Systematics provides an ideal subject because the science became historicized in stages. I analyze this historicization in light of my framework, comparing systematic reasoning in Lamarck versus Gegenbaur (5.2.1). I then analyze Hennig's development of phylogenetic systematics as a formalization of the historical framework (5.2.2). Some systematists – “pattern cladists” *sensu* Ereshefsky (2001), “transformed cladists” *sensu* Scott-Ram (1990), such as Schuh and Brower (2009) – have argued that the modern systematic methodology that developed following Hennig (1966) does not assume that evolution occurs. I show how cladistic methodology presumes that the natural classification of organisms reflects a pattern of historical genealogical descent. I illustrate by reference to a particular systematic study, the *Bassaricyon* case, which will be further explored in chapter 6. Whereas the present chapter primarily concerns what systematics hypotheses express, chapter 6 will address the relationship of evidence to systematics hypotheses.

## 5.1 HISTORICAL HYPOTHESES AND CAUSAL DEPENDENCE

### 5.1.1 Historical Causal Reasoning

Whewell grouped together those sciences that concern historical causation:

“Force is the *cause* of motion, because force at all times and under all circumstances, if not counteracted, produces motion; but the cause of the present condition and elevation of the Alps, whatever it was, was manifested in a series of events of which each happened but once, and occupied its proper place in the series of time. The former is *mechanical*, the latter *historical, cause*.” (Whewell, 1847b, p. 654).

Whewell then sketched the task of the historical sciences.

“In our present investigations, we consider the events which we contemplate, of whatever order they be, as forming a chain which is extended from the beginning of things down to the present time; and the causes of which we now speak are those which connect the successive links of this chain. Every occurrence which has taken place in the history of the solar system, or the earth, or its vegetable and animal creation, or man, has been at the same time effect and cause; - the effect of what preceded, the cause of what succeeded. By being effect and cause, it has occupied some certain portion of time; and the times which have thus been occupied by effects and causes, summed up and taken altogether, make up the total of Past Time. The Past has been a series of events connected by this historical causation, and the Present is the last term of this series. The problem in the palætiological sciences, with which we are here concerned, is, to determine the manner in which each term is derived from the preceding, and thus, if possible, to calculate backwards to the origin of the series.” (Whewell, 1847b, pp. 654-655).

Whewell introduced the chain analogy as part of his strategy for arguing two points: first, that historical science is *causal*; and second, that historical causation is an idea distinct from mechanical causation. Whewell coined the term *palætiological* precisely to emphasize the causal (ætiological) nature of historical science (Whewell, 1840b, p. 96). Mechanical causation (or "permanent causality" - Whewell, 1837c, p. 481) encompasses the various types of forces that operate in the same manner, regardless of time or location, and as such are describable by laws of causes. By contrast, historical

or “progressive” causation addresses causes that have “occupied some certain portion of time” as spatiotemporal particulars of interest in themselves, rather than as tokens of types of causes (Whewell, 1837c, p. 481).<sup>33</sup>

The idea of historical causation entails an ordered state of affairs. Whatever events, facts, and causal transactions obtained, according to a historical hypothesis, obtained in an ordered manner, though the ordering is not itself a matter of universal laws of causes. Historical scientists draw on evidence from diverse sciences that deal with permanent causes, each of which individuates entities in different ways (in keeping with the sciences’ distinctive conceptual frameworks). The different permanent sciences need not agree about what are causally relevant entities in the world, and the historical science need not form positive hypotheses identifying individual events and causal transaction links in the past. Instead the historical science posits *that* there has been some one story in the world, involving a single set of events that has an ordered continuity. The challenge is to establish facts about this historical story. Such facts are informative about past events, processes, objects, and states of affairs, and about past causal relationships. How to go about discovering such facts? Whewell described three components of historical science (1837c, p. 488).

The classificatory component requires historical scientists to delineate the target phenomena and develop terminology to describe the phenomena. This is not a trivial task. For example, in developing a classification of geological strata (Cambrian, Ordovician, and so forth), geologists had

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<sup>33</sup>Whewell’s chain analogy is a heuristic device intended to convey the two points explicated here and should not be mistaken for a claim that the essential task of historical science is to reconstruct event transactions like links in a chain. His sensitivity to the “hard part” relevance objection, handling of the chain “links” as non-repeatable particulars, and scheme for using causal content from disparate scientific domains involving different concepts, indicate that positive reconstruction of chains of events as causal transactions cannot be the whole story. This point will become clear through explication of his philosophy of the historical sciences.

to establish that strata occurring at different locations and under different conditions are meaningfully the same.

The second, ætiological component draws on disparate non-historical sciences to investigate the limits of permanent causes. The third component reconciles these contributions to form a *theory of the facts* that expresses what must transpired in the history of the target phenomena.<sup>34</sup> Whewell is less explicit than could be hoped as to the precise form of such a theory of the facts. Indeed he wrote that there is no extant complete theory of the facts in any palætiological science, and expressed doubts about the ability of historical science to produce any complete and satisfactory theory (Whewell, 1840b, p. 122).<sup>35</sup> Nonetheless, some successful historical theorizing had been

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<sup>34</sup>Whewell presented a tripartite division of branches of palætiological sciences in his *History of the Inductive Sciences*, for example in the case of geology: 1. phenomenal geology, dealing with classification and laws of phenomena; 2. geological dynamics, concerning the ætiological component; and 3. theoretical geology (sometimes called physical geology). In the *Philosophy of the Inductive Sciences*, Whewell avoided the use of the term “dynamics” as a stage or auxiliary component of the palætiological sciences, explaining that this phrasing too closely suggests that all the causes expressed by historical sciences are mechanical causes (Whewell, 1840b, p. 102). Whewell here described the ætiological stage as a separate body of science. In the *Philosophy* Whewell also shifted away from the straightforward analogy of geology to astronomy that he made in the *History* (Kepler: Galileo: Newton = Phenomenal: Dynamic: Theoretical).

<sup>35</sup>Whewell tied his skepticism to his conviction that key historical phenomena have had supernatural causes (Whewell, 1837c, p. 588; 1840b, p. 164). However, Whewell did seriously entertain origins hypotheses and require that they be rejected on scientific rather than theological grounds. For example, Whewell’s religious convictions were not responsible for his rejection of Darwin’s theory, as Ruse (2000) has suggested. In an 1860 letter to Darwin, Whewell wrote “there is so much of thought and fact in what you have written that it is not to be contradicted without a careful selection of the ground and manner of the dissent” (Charles Darwin, 1993). Whewell provided his own dissent in the form of empirical and methodological criticisms in a preface added to the 7<sup>th</sup> edition of his Bridgewater treatise (Whewell, 1864). The criticisms drew on his earlier arguments against transformation (Whewell, 1837c, pp. 573-580). Snyder’s (1994) work on Whewell’s views on necessity provides the grounds to counter Hull’s (1973) claim that Whewell’s epistemology compelled him to reject Darwin’s theory.

For the most part, historical speculation about origins of species, planets, and other entities had been conducted in a haphazard manner. Whewell likely hoped his philosophical explication of historical science would assist the organization of the historical sciences. At the time, the sciences

achieved, and Whewell himself sketched a theory of the facts about the history of Gothic architecture (Whewell & von Lassaulx, 1842 - see 1.2 below).

The theory of the facts expresses claims about what must have happened, and includes causal content, but not about what must happen between any token events of a given type, i.e. in the form of Millian-style laws. On Whewell's view the full development of a non-historical science results in formulation of laws of permanent causes. A non-historical science "if perfected, would be a demonstrative science dealing with general cases..." and would express "what always must be under given conditions" (Whewell, 1837c, p. 548). Whewell explicitly contrasted such laws with the causal knowledge expressed by the historical sciences, "an ætiological view having reference to special facts" (Whewell, 1837c, p. 548). These facts express "what is and has been, and why it has been" (Whewell, 1837c, p. 548). On Whewell's view, the most advanced historical sciences produce theories that involve particular facts about the past and causal information about why those particular facts obtain. Here is seen the same worry about historical causal reasoning that would recur in twentieth century debates: how can the historical scientist make causal claims about past particular states of affairs, except by reference to some generalizable principle?

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that target the past were in nascent stages of professional, methodological, and theoretical development (Tucker, 2004).

Chambers' evolutionary speculations cherry-picked causal claims from many different sciences without the careful combinatory reasoning that Whewell analyzed as the Theoretical stage. Moreover, Chambers advanced many of these individual causal claims without sufficient ætiological work. Whewell was particularly scathing of Chambers' highly scattered approach. Far more politely in tone, Whewell criticized Lyell's (1830) anti-catastrophism on similar grounds. Whewell claimed that Lyell has not fully established what individual causes can do (the ætiological work), because Lyell assumed that observed short-term effects can be extended indefinitely by the passage of time. Whewell admitted that catastrophism suffers from the same epistemic shortfall, that we do not know the possible limits of permanent causes. Whewell concluded that neither the catastrophist nor the gradualist theory is sufficiently supported (Whewell, 1831; 1837c, p. 616).

The theory of the facts expresses historical causal dependence relations describing particular cases. Regularities about historical causal dependence posit that an event story exists in the past of the cited entity or entities, and that the events constitute a single set with a continuous order. It is not necessary to reconstruct this past set of events directly in order to reason about dependence relations that exist among entities that descend from the single story.

The idea recalls Franklin and Franklin's (1888) attempt to ground Mill's view on natural kinds (chapter 2). In Franklin and Franklin's explication, members of Milleian kinds share properties as a result of some particular shared historical origin. We can call this historical causal dependence. Historically causally dependent properties are necessarily shared because they co-occur as a result of shared history – not just a similar type of origin story, but a shared particular past event story.<sup>36</sup> The idea contrasts with direct causal dependence, the relation that links properties on other accounts of natural kinds. Samples of gold have the properties of ductility, malleability, shininess, and so forth because these macroproperties result directly from the core properties of gold, such as its atomic configuration.

These senses of causal dependence are not confined to the context of natural kinds. For example the regularity, “native-born Cubans speak Spanish”, expresses that a property (Spanish-speaking) holds of a group (native-born Cubans) without claiming that this group does or does not constitute a natural kind. The regularity holds (approximately) true as a result of historical causal dependence. There is some set of events in the historical past that leads to the current state of affairs in which members of the specified group share the specified property. There are some exceptions to the regularity, owing to discoverable historical particulars.

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<sup>36</sup>This historical causal dependence links the properties that occur as clusters in Griffiths' (1999) historical essences view of natural kinds.

To illustrate the role of historical and direct property dependence in historical causal reasoning, I turn now to Whewell's own foray into the practice of historical science.

### 5.1.2 Historical Hypotheses and Whewell's Practice of Historical Architecture

Whewell's theory of the history of Gothic architecture exemplifies how historical sciences provide information about causal dependence of facts. Whewell intended his *Architectural Notes on German Churches* (1830) to contribute to the organization of the study of architecture as a palætiological science.<sup>37</sup> The book proposes standards for terminology, lists features that should be observed and noted, and provides worksheets that can be used to collect standardized information from observers in a variety of geographic locations. With the use of his own observations, Whewell sketched the

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<sup>37</sup>Snyder described Whewell's *Architectural Notes on German Churches* in relation to the development of his concept-focused philosophy of science, and as an attempt to ground a new, architectural science (Snyder, 2011, pp. 254-256). Snyder related this new science to the classificatory rather than the palætiological sciences, citing the passages in which Whewell expressly compared architecture to botany. Whewell indicated that one function of the *Architectural Notes* is to enable readers to discriminate between Gothic and other styles (that is, to grasp principles of architectural classification). The classificatory comparison corresponds to the classificatory component of architecture as a palætiological science. Whewell expected that botany would proceed through physiological reasoning guided by the idea of final cause, in addition to the classificatory concept of likeness. He explicitly described architecture as a palætiological science guided by the idea of historical cause (Whewell, 1840b, pp. 96, 113). The original Preface of *Architectural Notes* discusses methodology and epistemic problems associated with historical science, as distinguished by uniquely historical causal reasoning.

In the Preface to the third edition (1842), Whewell pressed the importance of the fact that his contemporaries had only recently agreed that Gothic architecture constitutes a unified whole; a singular entity within an architectural classification. Indeed he continued the argument in favor of the point. But the importance of this claim is that it is prerequisite for the primary study he undertook in his book, which targets "the conditions and causes of its [the Gothic style's] rise and progress; so long as they did not perceive clearly *what* it was, they could not discern *how* it came to be; so long as they did not understand the language of Gothic Architecture, they could not trace its phrases to their roots." (Whewell & von Lassaulx, 1842, p. 3). The central theory that the book seeks is an account of the historical origin and development of Gothic architecture as a coherent style.

historical progression leading to the full-fledged Gothic style of architecture. The theory aims not merely to hypothesize what might have happened, but to describe what must have happened.

The origin of the pointed arch would prove crucial to Whewell's theory. He contrasted his account with earlier speculations about the origin of the pointed arch:

“...the theory which I am now to develop, pretends not only to shew how this arch *might* be invented; but that it, or something like it, *must* have been wanting, discovered and employed.” (Whewell, 1830, p. 9).

Whewell proceeded to describe the effects that followed from the adoption of the pointed arch.

“That this adoption of the pointed arch led to the other changes that combine to form the Gothic style, is not capable of being proved with the same cogency. But we will trace a natural and almost necessary influence of this element upon the other parts of the building, which seems to explain better than any other hypothesis the formation of the new style.” (Whewell, 1830, p. 9).

Whewell traced the effects of adoption of the arch via reasoning about dependent relationships between church properties.

“...vaulting a space that has different length and breadth can only be effected by abandoning the semi-circular arch. The forms that the vaulting assumed when this arch ceased to be exclusively employed, were various. They were, moreover, variously affected by the distribution of the other parts of the building. I shall consider the consequence and progress of this combination of causes.” (Whewell, 1830, p. 10).

The consequence relationships that Whewell subsequently considered include both direct property dependence and also historical causal dependence.

For example, consider the task of constructing a vault over a square space. A space with equal width and breadth can be vaulted by constructing two equal semicircular vaults of equal height, bounded by two semicircular transverse arches and two semicircular longitudinal arches. Figure 5.1 reproduces Whewell's sketch, including a square diagram of Whewell's notation for this arrangement. The vault is semicircular, and each edge of the space is a semicircular arch of equal height to the vault.



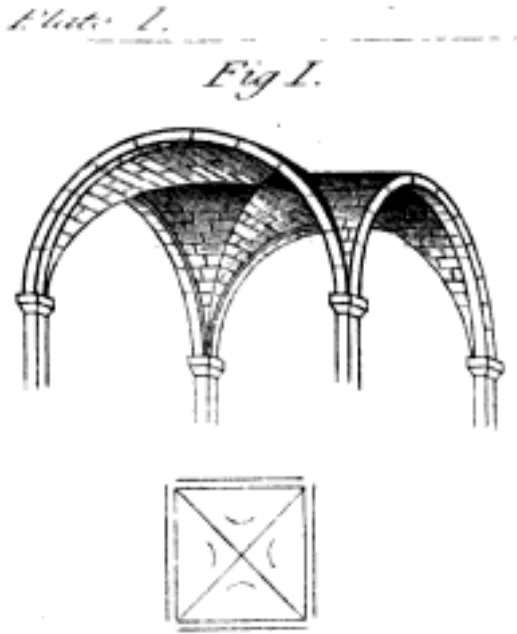


Figure 5.1. A square space with equal semicircular arches. Whewell's (1830) plate I, figure 1.

Now consider vaulting a rectangular space that is not a square. It is impossible to vault the space using intersecting semicircular vaults of equal height:

“...the transverse vault will not reach to the top of the longitudinal one, but will cut it obliquely in an irregular curve: the line running across the top from one window to its opposite, will be a broken line: the forms will be of some complexity to calculate, and can only be executed by great skill, and with much difficulty.” (Whewell, 1830, p. 7).

Early, Romanesque style churches use only semicircular arches. Whewell reasoned that the use of only semicircular arches constrained the dimensions of each church compartment. Romanesque style churches are composed entirely of square compartments. This in turn constrained the relative width of distinct aisles in the church. Figure 5.2 shows how a Church with three aisles can utilize semicircular vaults and arches, provided that each side aisle is half the width of the main aisle, by doubling the number of spaces to be vaulted in each aisle (thereby ensuring that each space to be vaulted in the side aisles is square).

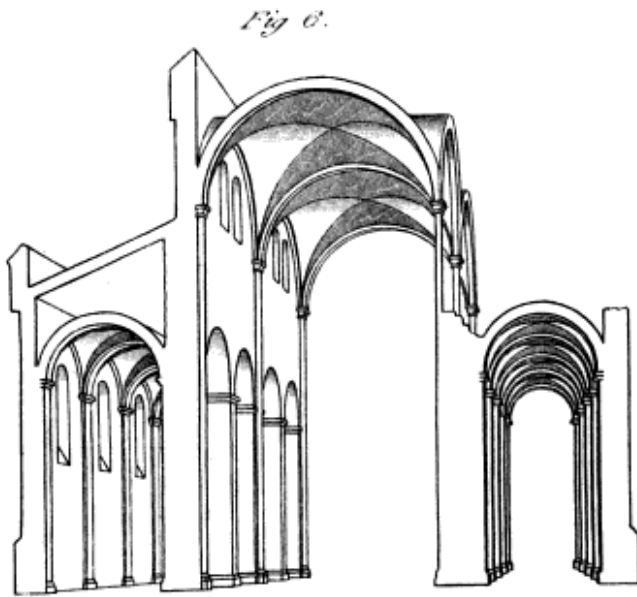


Figure 5.2. Church with two side aisles and only semicircular vaults. Whewell's (1830) plate IV, figure 6.

It is possible to construct a symmetrical vault with equal height across each opening by using a pointed arch for at least two sides (Figure 5.3).

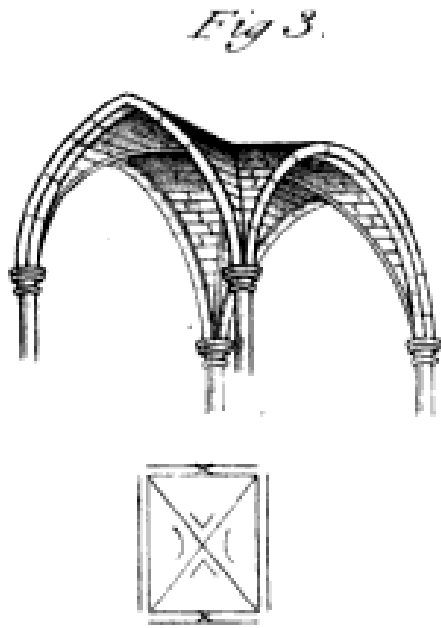


Figure 5.3. Vaulting a rectangular space using pointed and semicircular arches. Whewell's plate I, figure 3.

Symmetrical, equal height vaults can also be constructed by abandoning the semicircular arches in favor of pointed arches for both transverse and longitudinal arches. This became the standard Gothic arrangement. Whewell described the problem primarily in geometrical terms, though the arrangement turns out to accord well with mechanical and logistical constraints:

“It is not only that the forms and bearings of the parts of this arrangement are more easy and simple than any other which suggests itself, capable of answering the same ends, but also its strength is more easily secured, and the mechanical construction of a vault may be facilitated.” (Whewell, 1830, p. 8).

The use of the pointed arch is thus connected as a matter of direct property dependence to mechanical properties about the structure’s strength and distribution of load.

The possibility of vaulting non-square space has many ramifications on church structure. Whewell described at length how the development of the pointed vault with its pointed arches led to alterations in the number and placement of windows, the placement of columns, the arrangement of

capitals, and other modifications (see for example pp. 29-33).<sup>38</sup> These modifications directly relate to constraints imposed by preferred geometric relationships, requirements of lighting, and so forth, describable by analysis of architecture as both science and art.<sup>39</sup> The relations of direct causal dependence are describable via theories of distinct scientific (and aesthetic) domains.

Possession of some church characters is lawfully related to possession of others as a matter of historical causal dependence. Churches in the earliest Complete Gothic style tend to have geometrical tracing over the windows; later examples have flowing (curvilinear) tracery (Whewell, 1830, pp. 66-68). The character of flowing tracery is not directly related to the character of flying buttresses, but for historical reasons the two characters are linked. As a matter of historical dependence, we can predict that we will not find a case of a Church that has flowing tracery and round arches.

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<sup>38</sup>Whewell discussed the architectural feature that Gould and Lewontin (1979) used to illustrate their concept of an evolutionary *spandrel*. The space between arches may take a certain form by virtue of purely geometric relations that are directly dependent on the arch form. The spaces may come to have other causal dependence relations with other characters – in Gould and Lewontin’s example, by the development of a particular style of decorative painting that complements the basic spatial arrangement.

<sup>39</sup>Snyder (2006) discussed Whewell’s explanation of the unifying concept of *verticality* as dictating the other elements of the Gothic style, but her discussion suggests that these other elements are necessitated by the idea of vertical lines because of facts about distribution of weight and other engineering requirements. Whewell does refer to such causal mechanical factors. However, the majority of the dependence relations that he describes cite symmetry and geometry without suggesting any underlying mechanical necessities. In many cases it is clear that engineering constraints do not play a role.

The facts of symmetry and geometry must somehow reflect underlying causal laws, but the nature of the relevant type of cause was as yet unclear. Whewell (1840b, pp. 116-120) discussed the types of causes to be cultivated in the ætiological stages of the palætiological sciences for each of geology, glossology, and biogeography, but not historical architecture, notwithstanding that he had used the origin of Gothic architecture as an example in arguing for the necessity of developing an ætiology (p. 113).

Property dependence relations are evidentiary towards the nature and order of the pattern changes that Whewell describes. By explicating the property causal relationships, Whewell is able to explain why his proposed theory of the facts not only could have happened but (if he has reasoned well) why the specified changes, or something like them, *must* have occurred.

Two aspects of Whewell's reasoning are crucial. First, Whewell described each church as a system of inter-dependent parts. He reasoned about these inter-dependencies and the kinds of change that are possible within the system. Second, he put this reasoning in historical terms: some changes would necessarily have preceded other changes. Once an architect is using the pointed arch, she can change the spatial relationships between main aisle and side aisle; the number of side compartments; the shape of the vaulting; and so on, while preserving the symmetry to the church. The remainder of *Architectural Notes* describes these and other changes, fitting them into place relative to each other. The result is a theory of the facts that traces the development of Gothic architecture in terms of the changing relationships of parts of church structure.

A historical system has a "memory": the situation at any point in time depends on the situation at prior points in time. Historical dependence relationships cannot be imposed on the tree "from scratch" but instead develop in a continuous way. The story of a historical lineage is a story of transmission and alterations of a coherent set of possibilities. This does not imply that change operates only in one direction; reversion to prior stages may be among the set of possibilities. The precise limits on possibilities are an empirical matter.

The historical story of interest is a single, coherent story. The required spatiotemporal continuity of the historical story is a minimal constraint on historical hypotheses. In the case of Gothic architecture, historical hypotheses are constrained by mechanisms for communication. A historical hypothesis could not link German and pre-Columbian Aztec architectural styles unless the hypothesis included claims of pre-Columbian contacts between Aztec and German societies.

In the case of biological organisms, historical hypotheses are constrained by mechanisms of reproduction of the organisms and mechanisms of transmission of the evidence about biological relationships. In the next section I explicate these constraints to show what changed when systematic hypotheses came to be thought of in historical terms. As will be seen, Lamarck's system illustrates an incomplete historicization of systematics; Gegenbaur's work exemplifies the historical reasoning explicated in the above section; and Hennig's methods formalize this historical reasoning.

## 5.2 SYSTEMATICS AS HISTORICAL SCIENCE

### 5.2.1 Lamarck and Gegenbaur

Lamarck (1809) proposed a system wherein the state of an individual organismal lineage at each historical time depends on the state of that lineage at the preceding time. However, his systematic groupings were not historical hypotheses in the sense described above. Lamarck held that the least complex organisms arise all the time as a result of spontaneous generation. Each lineage thus generated evolves toward greater complexity, but along the very same pathway as prior lineages. That is, the newly generated lineage will pass through each of the worm, radiarian<sup>40</sup>, insect, and

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<sup>40</sup>Lamarck grouped jellyfish, anemones, and relatives (the modern Cnidaria) with starfish and relatives (Echinodermata) as the radiarians. Lamarck amended Linnaeus' classification by distinguishing these taxa from the Mollusca, which are not closely related to either cnidarians or echinoderms. In fact, echinoderms are more closely related to molluscs than they are to cnidarians. However a monophyletic group containing both echinoderms and molluscs must also contain vertebrates, insects, annelids, and various other taxa that both Lamarck and Linnaeus sought to distinguish from the starfish and molluscs (Tree of Life Web Project, 2002).

subsequent stages. In Lamarck's system, radiarians arose and continues to arise as a polyphyletic group, not as the result of any single connected historical story.<sup>41</sup>

The situation is analogous to the idea that Gothic architecture should include structures built by Aztecs and by Germans, so that the Gothic style arose and developed via multiple separate origins without an underlying shared historical story. The separate Aztec and German lineages might proceed with historical continuity, but the grouping of these lineages together does not satisfy the classificatory requirements of historical science. A "Gothic" architecture that encompassed both lineages would not be a historical entity.

By contrast, Gegenbaur's 1878 *Elements of Comparative Anatomy*<sup>42</sup> makes systematic claims as historical hypotheses. Gegenbaur sought "the morphological explanation of the phenomena of form met with in the organization of the animal body" (Gegenbaur, 1878). He proceeded systematically through (what he argued to be) the natural system. Whereas Candolle had reasoned about transformations at local levels (see chapter 6), Gegenbaur arranged his entire text as a progressive build-up of morphological systems over time.<sup>43</sup> The sense of "originally" and

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<sup>41</sup>We will have to define "historical story" in some sufficiently localized fashion so as to preclude stories that cite the whole state of the earth at a specified time. Lamarck seemed to hold that each lineage's characters will be identical because they result from the successive operation of the same type of cause rather than the same particular historical cause. On Lamarck's system, when characters arise via a different type of cause – adaptation – organisms diverge from the regular form.

Another possible view would be to adopt Cuvier's strong "conditions of existence" doctrine, under which each lineage would have to pass through the same stages because these are the only physically possible configurations. Groups united by possession of these characters need not share a single united historical story. Such a strong version of Cuvier's conditions of existence doctrine is empirically false.

<sup>42</sup>The first German edition was published in 1859.

<sup>43</sup>The form of exposition – a long progression from the simplest organisms to the most complex – is reminiscent of explications of the Great Chain of Being. In Gegenbaur's text the exposition results from evidentiary and explanatory concerns. Gegenbaur reasons from the continuity principle that the most complex organisms must be intelligible as complications of prior, less complex stages. Extant simpler organisms provide some evidence at least of what simpler systems are possible. By

“transformed” in Candolle’s work is unclear (see page 133), but Gegenbaur explicitly presumed that affinities between organisms result from shared descent (Gegenbaur, 1878, pp. 8-9). In Gegenbaur’s approach, ontogenetic study informs systematic and morphological theorizing, but not solely through explicit application of any set ontogenetic law to exemplifying instances.<sup>44</sup> Gegenbaur reasoned about developmental resources and how developmental systems can be reconfigured to result in morphological transformations. The overall progressive organization of the book reflects the build-up of progressively more complex systems that utilize progressively larger sets of resources. In reasoning about the nature and systematic value of a morphological structure, Gegenbaur considered each part not in isolation but in terms of hypotheses of its historical connection to earlier developmental systems.

For example, Gegenbaur addressed the origin of muscle and nerve tissue in order to understand the meaning of muscular and nervous characters as systematic evidence. Reasoning directly from functional considerations and natural selection to the origin of muscle and nerve tissue would be difficult. It seems that the two independent types of tissue must evolve simultaneously in order for either to fulfill its function (contractility for muscle tissue; sensitivity to stimuli for nerve tissue), and an explanation of either tissue’s origin in terms of its function cannot explain the coincidence of both tissues having arisen simultaneously. Gegenbaur reasoned in terms of available ontogenetic resources and processes. A present adult organism which has both these types of tissue

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accepting what is now called Haeckel’s biogenetic law, Gegenbaur also accepted that currently extant simple organisms demonstrate actual ontogenetic stages and hypothesized historical adult stages of more complex organisms.

<sup>44</sup>Gegenbaur claimed that the ontogeny of more complex organisms “to a certain degree” recapitulates the adult stages of less complex organisms (p. 6). For example, Gegenbaur identified the adult form of contemporary Cnidarian nerve-muscular tissue with a hypothesized past stage in his phylogenetic historical narrative. This is the biogenetic law that would come to be associated with Haeckel (with whom Gegenbaur worked). Application of this law is a narrower form of reasoning than Gegenbaur’s general historical reasoning about developmental systems.



must have acquired them through some developmental transition of an earlier stage. He hypothesized that a single type of neuro-muscular tissue gradually became specialized, culminating in the origin of distinct nervous and muscular tissue via the splitting of a single layer, the ectoderm (pp. 30-31). On this hypothesis, possession of one type of tissue is linked to possession of the other type of tissue through historical causal dependence within the timeframe of individual ontogeny. Neither tissue type could have arisen in the organism without the production of the other type, although one tissue type could be lost subsequent to the splitting. Possession of one type is not a matter of direct property dependence on possession of the other. The key to Gegenbaur's reasoning is to envision nerve and muscle tissue not as entirely new tissue types with wholly new functions, but rather as differentiated and further developed products of a store of developmental resources. Gegenbaur could then interpret the acquisition or loss of these tissue types in historical hypotheses about the build-up and alteration of this developmental store.

Loss of characters and the complete loss of parts may be among the possible transitions of a given developmental system. Gegenbaur held that reversion to less complex states is rarer than progressive increase in complexity because of a division of labor principle (Gegenbaur, 1878, p. 14). Gegenbaur claimed that the more specifically a part is associated with a function, and the function localized to a single part, the better the performance of the function. Consequently, adaptation will lead to increased specialization of parts and division of labor into a greater number of parts. Once a function is localized to a part, an organism cannot lose the part without losing the function and whatever adaptive advantage possession of the function conveys. On Gegenbaur's system, adaptation will tend to produce organisms with a greater number of more specialized parts, that is, of greater complexity.

Gegenbaur's work demonstrates historical causal reasoning in the domain of systematics, but the discipline did not immediately adopt a thoroughly historical scientific framework. This

framework was adopted in conjunction with the twentieth century development of formal methods for expressing systematic hypotheses. Willi Hennig (1966) provided a clear explication and forceful defense of these methods, to which I now turn.

### 5.2.2 Hennig's Methodology and Tree Diagrams

Hennig argued for the priority of an explicitly historical systematics as a logical matter. Systematics seeks to reconstruct history, and its hypotheses and methods should presume that the task is historical. This can be seen in Hennig's redefinition of monophyletic groups (clades) and in his development of the concepts of plesiomorphic and apomorphic characters.<sup>45</sup>

Early twentieth century systematists sought to identify groups termed "monophyletic" in the sense of sharing a common ancestor. These were deemed the natural groups, in contrast to polyphyletic taxa. Polyphyletic taxa are groupings of entities that have distinct origins. The members of a polyphyletic grouping do not share any single common ancestral lineage. Hennig recognized that this way of distinguishing polyphyletic from monophyletic groups was deeply problematic. All life on earth is thought to descend from a single common origin. Thus every possible grouping of organisms shares some common ancestor, somewhere on the tree of life, and is monophyletic in the older sense (Hennig, 1975, p. 247).<sup>46</sup>

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<sup>45</sup>Mayr (1982) and N. Eldredge (1979) identified the apomorphic/plesiomorphic distinction as Hennig's key contribution to systematics. Hennig's redefinition of monophyly implies the need for these character concepts.

<sup>46</sup>In the early twentieth century there were empirical disputes about distinct origins of the same taxa, but the concepts of monophyly and polyphyly as defined during this time did not capture the issue. At issue was whether distinct populations of organisms acquire the same evolutionary novelties, subsequently come into contact, and form a single lineage with multiple distinct historical origins. Bather (1927) discussed this question in terms of the polyphyletic origin of taxa; the members of such taxa would still share a common ancestor further back in the tree of life.

Hennig redefined monophyletic groups as those that contain an ancestor and all and only its descendants (Hennig, 1966, p. 73). This stricter sense distinguishes monophyletic groups from what he identified as paraphyletic groups as well as polyphyletic groups. A paraphyletic group includes an ancestor and some but not all of its descendants. A polyphyletic group does not include any single ancestor shared by all of its members. Figure 5.4 illustrates mono-, para-, and polyphyletic groups.

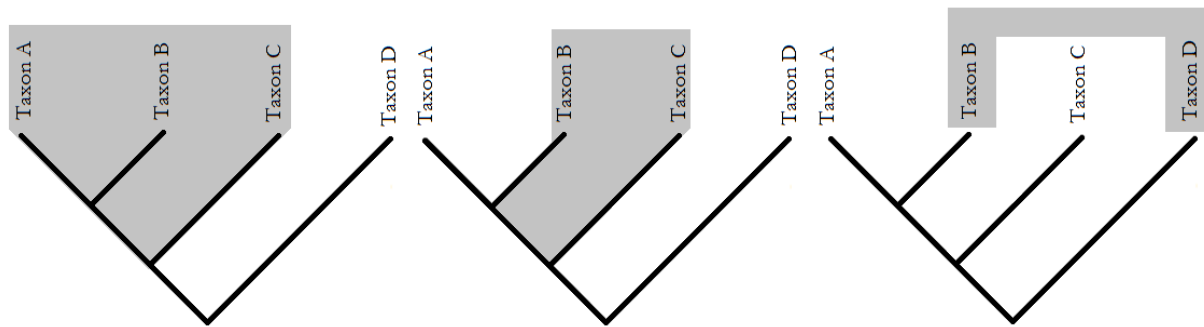


Figure 5.4. Gray shading indicates proposed groupings. At left, a monophyletic group (clade) that includes taxa A, B, and C. Center, a paraphyletic group containing B and C but not A. These taxa are united through their descent from an ancestor that is not shared by taxon D. However, B is more closely related to A than either A or B is to C. The paraphyletic group contains some but not all of the descendants of the common ancestor of B and C. At right, a polyphyletic group. B and D do not share any common ancestor to the exclusion of other taxa. B and D have some common ancestor, as do any two natural taxa in the tree of life, but the proposed grouping cannot be defined using a common ancestor.

Hennig's redefinition makes group hypotheses into claims with particular empirical content about the historical relationships of organisms. Note that paraphyly and polyphyly can only be distinguished by reference to the placement of an ancestor. To show this, Figure 5.5 redefines the paraphyletic group from the center of Figure 5.4 as a polyphyletic group. Figure 5.6 redefines the polyphyletic group from the right of Figure 5.4 as a paraphyletic group. The redefinitions are achieved by reference to inclusion or exclusion of hypothesized ancestral taxa.

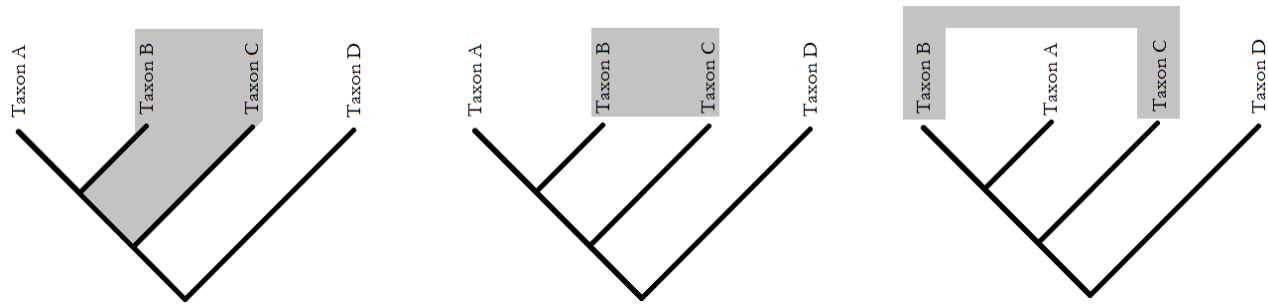


Figure 5.5. At left, the paraphyletic group that includes [B, C] from the center of Figure 5.4. At center, the common ancestor of [A, B, C] is excluded from the proposed group. Group [B, C] is now polyphyletic. The vertices of tree diagrams can be rotated; the diagram at right is equivalent to the diagram at center.

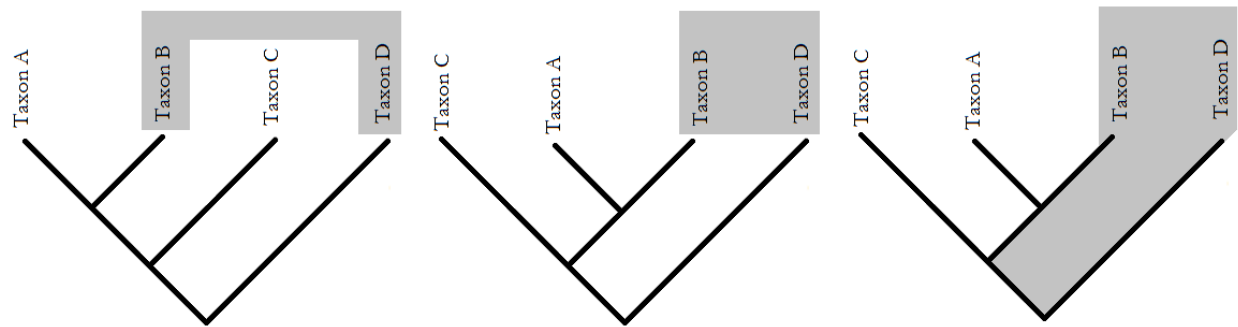


Figure 5.6. At left, the polyphyletic group [B, D] from the right of Figure 5.4. Vertices can be rotated to yield the equivalent diagram at center. At right, by including the common ancestor of [A, B, C, D], but still excluding A and C, the group [B, D] is rendered paraphyletic.

In connection with his distinction between mono-, para-, and polyphyletic groups, Hennig defined concepts for similarity that might be observed across groups. Observed similarities may be synapomorphic, symplesiomorphic, or homoplasious. Homoplasious characters arise separately in distinct lineages. In most cases of polyphyletic taxa, the taxon was defined on the basis of characters that were thought to be shared as synapomorphy but which were in fact homoplasious.<sup>47</sup>

<sup>47</sup>The typical situation is that systematists name a taxon with the intent of describing a natural group. Subsequently, systematists may discover that the taxon is paraphyletic or polyphyletic. I say

Synapomorphic characters are derived with respect to a monophyletic group: the character arose in the ancestor of the clade. Symplesiomorphic similarity results from inheritance of an ancestral state that is not derived with respect to the proposed grouping. In most cases of paraphyletic taxa, the taxon was delineated on the basis of similarities that were in fact symplesiomorphic. Figure 5.7 depicts synapomorphic, symplesiomorphic, and homoplasious similarity, and (respectively) monophyletic, paraphyletic, and polyphyletic groups.

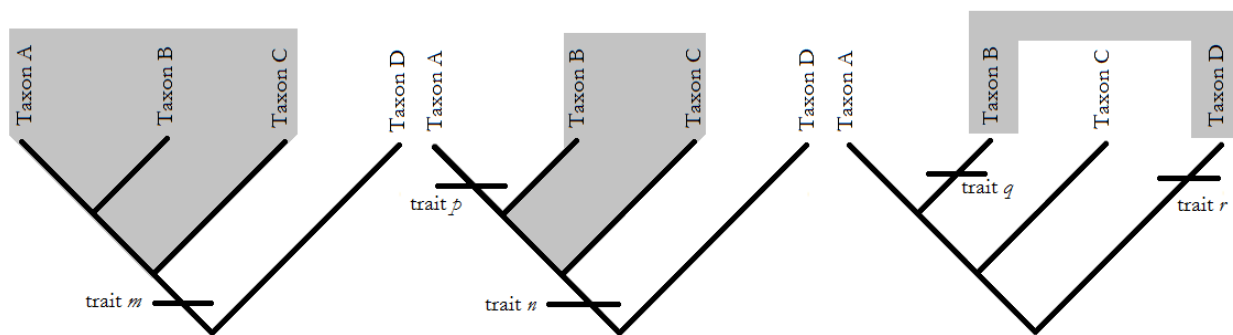


Figure 5.7. At left, A, B, C share trait *m* as a result of its origin in an ancestor of the monophyletic group [A, B, C]. Possession of *m* is synapomorphic similarity. At center, trait *p* is autapomorphic in taxon A. Trait *n* arose in an ancestor of A, B, and C and would be synapomorphic with respect to that grouping. The depicted grouping [B, C] is paraphyletic. The members of B and C resemble each other through possession of *n*, but if this similarity is used to define the paraphyletic group that excludes taxon A, the similarity is symplesiomorphic. Trait *p* might entail the disguise or loss of trait *n* so that *n*'s occurrence in B and C is perceived as a similarity between B and C to the exclusion of A. At right, trait *q* arose as an autapomorphy in taxon B and trait *r* as an autapomorphy in taxon D. If traits *q* and *r* are perceived as the same character state, the similarity is homoplasious, having arisen through convergent evolution in the distinct lineages leading to B and to D.

Tree diagrams such as Figure 5.4 can be read in various ways. The lines (branches) may be thought of as biological lineages. Alternatively, nodes may identify biological entities (e.g. taxa), with

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“most cases” to describe these situations. It is also possible to deliberately name paraphyletic or polyphyletic taxa, and to do so without any particular similarities in mind.

lines depicting relationships between nodes.<sup>48</sup> In Figures 5.4 through 5.7 terminals are taxa, but tree diagrams can be used to depict organisms, populations, genes, and other biological entities. At the most basic level of phylogenetic analysis, a tree diagram indicates a hypothesized pattern of descent. The diagram can be thought of as simply showing a pattern of character possession observed in biological specimens. However, given that the depicted characters are hypothesized to have singular historical origins, the diagram necessarily expresses claims about the history of entities that bear the characters. If depicted characters are claimed to be synapomorphic, the pattern of synapomorphies entails a pattern of historical descent.

In distinguishing monophyly, paraphyly, and polyphyly, and in distinguishing between synapomorphic, symplesiomorphic, and homoplasious similarity, phylogeneticists<sup>49</sup> make claims about historical ancestors of the organisms in question. Phylogeneticists need not identify specific ancestors – the *bête noir* of the “pattern” or “transformed” cladists (Ebach, Morrone, & Williams, 2008; Ereshefsky, 2001). Pattern cladism arose as a movement within the systematics community

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<sup>48</sup>In the terminology of de Queiroz (2013) the former are lineage trees and the latter relationship trees. This distinction replaces earlier distinctions between stem- or branch-based and node-based trees (de Queiroz, 2007; Martin, Blackburn, & Wiley, 2010). My placement of characters along branches (rather than at nodes) in Figure 5.7 suggests that the branches represent lineages in this figure.

See N. Eldredge and Cracraft (1980) for helpful discussion. In their (influential) terminology, phylogenetic trees identify ancestors (as nodes or stems), whereas cladograms depict patterns of relationship among taxa without specifying ancestors.

<sup>49</sup>Philosophers sometimes refer to the methodology of systematics since the uptake of Hennig (1966) as *cladistic* (for example, Brigandt, 2011; Wilkins, 2012). In systematics literature, the term “cladistic” has become associated with a particular subset of methods (parsimony) and practitioners (that eschew maximum likelihood and Bayesian approaches). The phrase *phylogenetic systematics* has also (to a more limited extent) become associated with these methods and systematists (Baum & Shaw, 1995, pp. 290, fn 294; de Queiroz, 1998, p. 58). Following Wiley and Lieberman (2011), I will refer inclusively to the modern field of systematics as *phylogenetics*, and its practitioners as phylogeneticists. Contrary to Wiley and Lieberman (2011, pp. 201-202) I intend to include transformed cladistics and the methodology of three-taxon analysis (Nelson & Platnick, 1981; Scotland, 2000; Williams & Ebach, 2008) within phylogenetics.

concerned to root out unwarranted evolutionary assumptions in phylogenetic practice (see for example Cracraft, 1981). Prominent among the disputed assumptions was the claim that we can directly identify taxa as ancestors. In the most problematic cases, systematists identified taxa that are (or were) widespread, old, and/or deemed primitive (in some sense – sometimes through possession of character states thought to be ancestral, other times for largely subjective reasons) as ancestral to taxa of more recent origin. The pattern cladists' reaction was a much needed corrective still quite beneficial in some areas.

The pattern cladist program subsequently hardened to push for the exclusion of any evolutionary reasoning at all in systematics. However, cladists' use of the concepts of apomorphy and plesiomorphy entails an evolutionary perspective. In distinguishing between monophyletic, paraphyletic, and polyphyletic groups, phylogeneticists do make claims about the existence of an ancestor that is or is not included in the identified phylogenetic groups.

Phylogenetic hypotheses are historical in the sense of the framework I developed above (section 5.1.1). We saw in Gegenbaur (5.2.1) historical reasoning in Whewell's sense (5.1.2), treating organisms as systems of inter-dependent parts with characters linked through causal dependence. In Hennig's framework, this reasoning about organisms and character states occurs at the ontogenetic and tokogenetic levels, concerned with the transmission of characters within individual organisms and populations. The synapomorphic, symplesiomorphic, and homoplasious concepts enable the systematist to relate reasoning about characters to the phylogenetic level, as claims about characters expressed in phylogenetic groups. Tree diagrams express historical causal dependence relationships between characters and facts about lineages.

Both plesiomorphic and apomorphic characters are possessed as a result of history. When these characters are shared across groups as symplesiomorphy and synapomorphy, possession of the characters results from a shared history. In the case of synapomorphy, the character is shared as the

result of history that is unique to the entities in question. This is the evidence required to support hypotheses about groups. The adoption of Hennig's concepts made clear the sense in which similarity is causally related to hypotheses about the natural grouping of organisms. While Gegenbaur and others used historical causal reasoning to evaluate characters in individual cases, prior to the clarification of the concept of monophyly, the relationship between trait possession and systematic hypothesis was unclear. When expressing a claim in terms of apomorphic or plesiomorphic characters, the relationship of character claim to phylogenetic hypothesis is explicit.

Tree diagrams are historical hypotheses in the sense of section 5.1. Systematists presume that some single historical story has obtained, but need not express hypotheses identifying particular events and causal connections between events. Tree diagrams do express facts about the pattern of history and do restrict the set of possible past events. Goudge (1961) suggested that historical hypotheses typically support negative predictions, and Cleland (2011) argued that historical scientists primarily make predictions about evidence to be discovered. Phylogenetic diagrams support negative predictions about what evidence may be discovered. For example, the diagram in Figure 5.8 depicts relationships among extant species of genus *Bassaricyon*, the olingos and Olinguito, a small new-world carnivore in the raccoon family (Procyonidae).



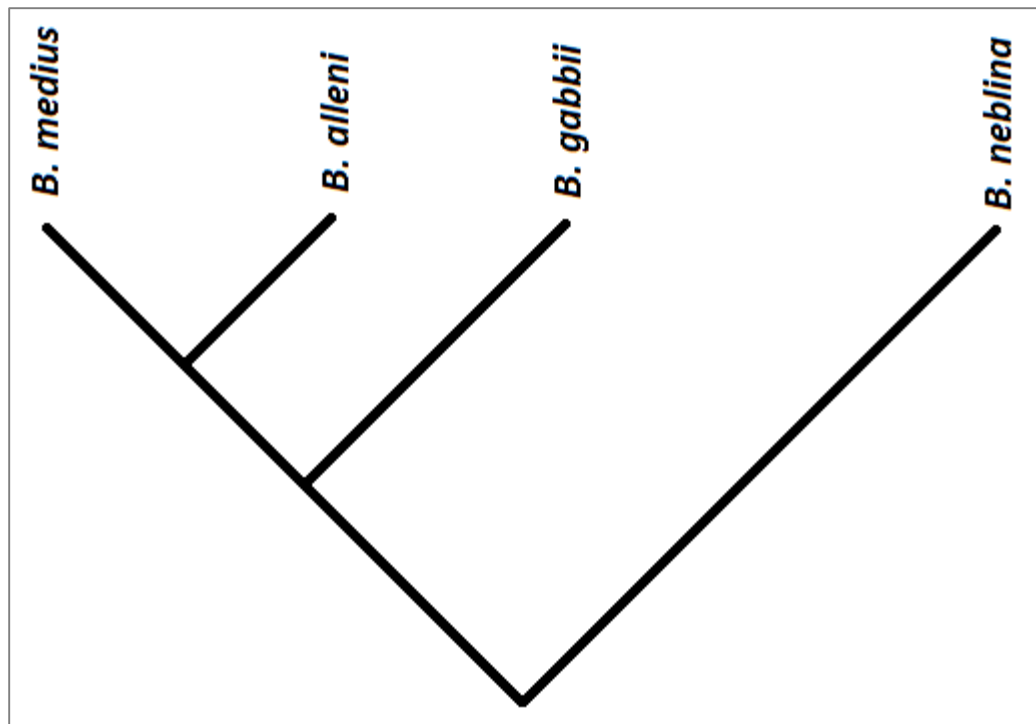


Figure 5.8. Relationships within *Bassaricyon*, the olingos and Olinguito. Helgen et al. 2013.

A particular deletion mutation, the birth of an olingo, a population migration, and an olingo's eating of a fig on some particular occasion are all bound up with the evolutionary history of the olingos, and these events will have occurred in ordered continuity. The diagram presumes the existence of a history that includes events such as these. The pattern of history expressed by the diagram rules out the possibility of certain events having occurred. *Bassaricyon alleni*, *B. medius*, and *B. gabbii* are hypothesized to be more closely related to each other than any of the three is to *B. neblina*. This expresses the constraint that there is no lineage that is more closely related to *B. neblina* and *B. gabbii* than it is to *B. alleni*. The hypothesis of Figure 5.8 rules out events that would be associated with the evolution of such a lineage. This constraint entails the prediction that we will not find any evidence descending from the existence of such a lineage. Figure 5.9, which is not a valid phylogenetic hypothesis, depicts such a proposed, impossible lineage with the name "*B. nihilo*".

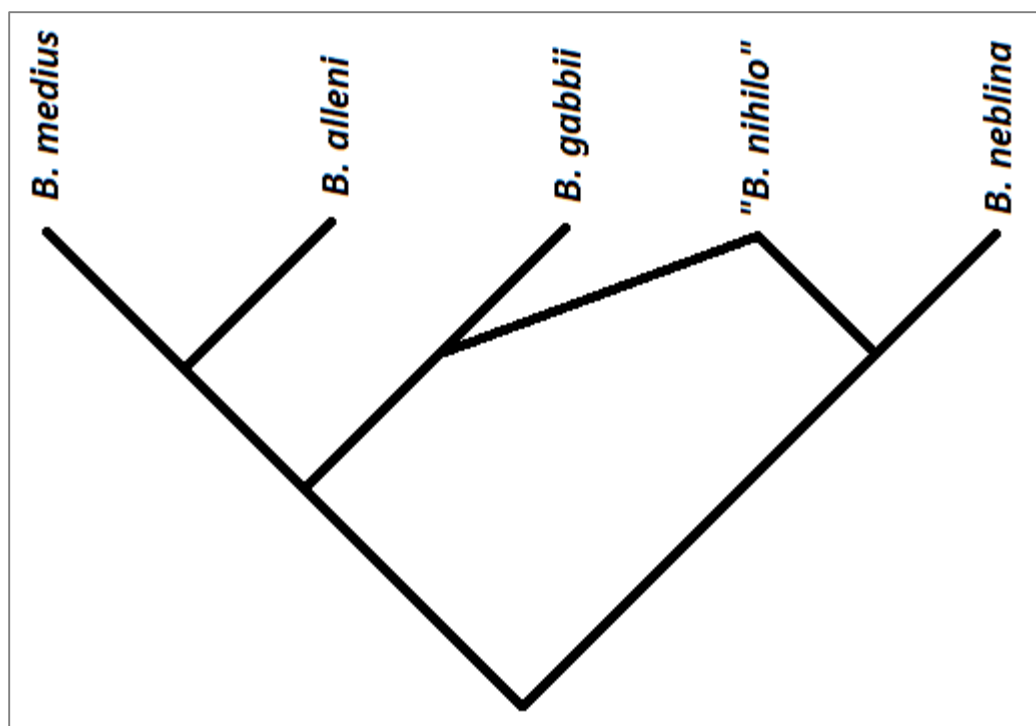


Figure 5.9. An invalid tree diagram. The diagram asserts the contradictory claims that *B. neblina* and "*B. nihilo*" are sister taxa and that *B. allenii* and "*B. nihilo*" are sister taxa.

In terms of characters, we will not find evidence of a lineage that shares the synapomorphic characters of *B. neblina* and the synapomorphic characters of any combination of *B. medius*, *B. gabbii*, and *B. allenii*. In Figure 5.10, *a* is an autapomorphic character with respect to *B. neblina*; *b* is a synapomorphic character with respect to the [*B. medius*, *B. gabbii*, *B. allenii*] clade; *c* is an autapomorphic character with respect to *B. gabbii*. The existence of "*B. nihilo*" would falsify the hypothesis that the organisms form the clades expressed in Figure 5.8, and also the individual hypotheses that characters *a*, *b*, and *c* are apomorphic.<sup>50</sup>

<sup>50</sup>The phrasing here is difficult. In order for a character to be apomorphic, it must be derived with respect to some clade. The existence of *B. nihilo* invalidates the clades expressed by Figure 5.8, so that there is no clade with respect to which character *b* could be derived.

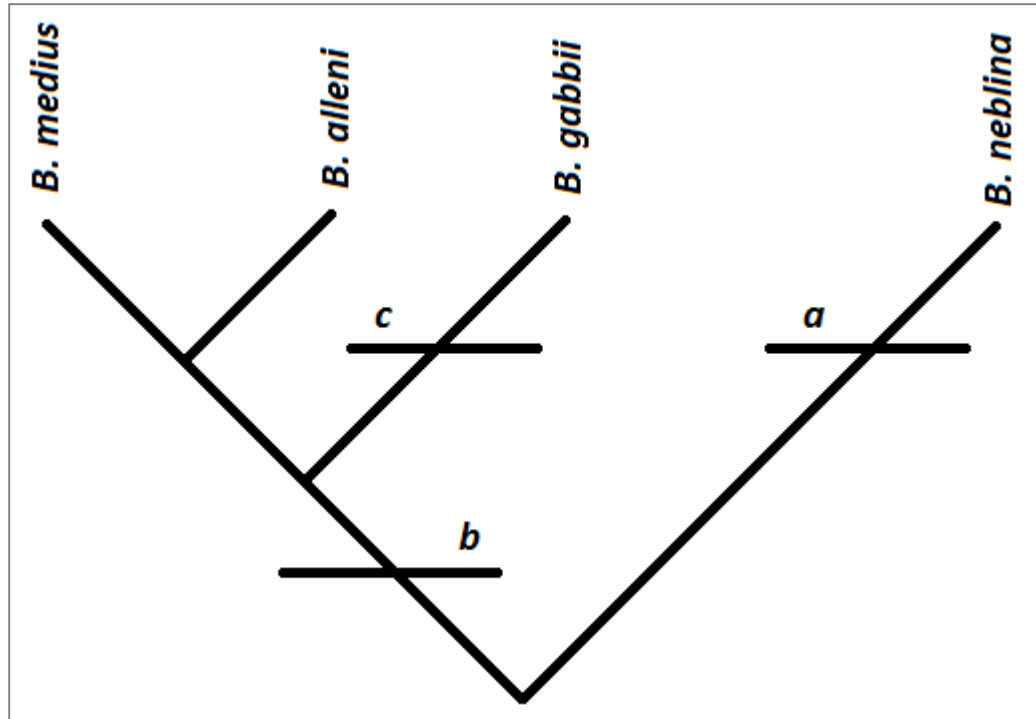


Figure 5.10. Apomorphic characters within *Bassaricyon*. *b* is a synapomorphy shared by *B. medius*, *B. alleni*, and *B. gabbi*; *a* is an autapomorphy of *B. neblina*; *c* is an autapomorphy of *B. gabbi*.

Phylogenetic diagrams present historically causally dependent facts. Taxa are posited to line up with the derived character patterns depicted by the cladogram, so that we assert that specimens belong to taxa and possess the characters in keeping with the depicted dependence relations.

Systematists group together those *Bassaricyon* specimens that have shorter tails (proportionate to body length) and larger upper first molar teeth, among other shared characters. Members of this grouping, designated *Bassaricyon neblina*, share these characters not because of any direct causal dependence relation (e.g. that having large molars reduces tail length). Systematists posit that there

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Characters can be derived with respect to single species. However, the concept of “derived” does not make sense if transmission of characters is possible in the reticulated sense implied by Figure 5.9.

has been some shared historical story underlying the grouping, such that a specimen with the shorter tail is hypothesized to have larger molars as a matter of historical dependence. If the specimen derives from the event story that included the events leading to the short tail, the specimen is claimed also to share those events that lead to the large molars. The assertion that a particular specimen bears the tail length character depicted in *B. neblina* implies that the specimen bears the molar character of that taxon.<sup>51</sup>

The basic elements of Hennig's framework, as I have argued in 5.2.2, match the framework for historical science developed in section 5.1. The most conceptually fundamental phylogenetic hypotheses depict patterns of relationship among entities. The causal information expressed by phylogenetic hypotheses typically takes the form of facts, including facts about causal dependence between characters. Hennig's basic framework has been adopted by almost all systematists, and has been hailed as a methodological revolution (for example by Nelson, 2004, p. 133; Wheeler, 2008, p. 15). The revolution was the introduction of explicitly historical concepts and methods such that phylogenetic hypotheses are essentially historical. I turn now to the nature of phylogenetic inference and the relationship between evidence and hypothesis.

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<sup>51</sup>This holds when one asserts that the observable character is the derived, *B. neblina* character. It is possible that some other character is observationally equivalent to this derived character – for example the tail length may bear the same proportion to body length in some other clade. Though observationally equivalent, this is not the *same* character (it is not homologous). A character that is observationally equivalent but does not bear identical historical dependence relations is not the same character.

## 6.0 PHYLOGENETIC INFERENCE TO THE BEST EXPLANATION

In this chapter I develop an account of phylogenetic inference as inference to the best explanation. Systematists accept as true the tree hypothesis that best explains the available evidence. My task is to fill in the details on two major issues about abductive inference and avoid two attendant objections to the adequacy of inference to the best explanation. First, where do candidate explanations come from, and how can we know that the true explanation is among the candidates considered? Second, what counts as the “best” explanation, and how can we answer this question without reducing abductive inference to tautology? The danger of tautology is that if we define the “best” explanation as the one likeliest to be true, abductive reasoning infers only that the explanation that is likeliest to be true is likeliest to be true. I argue that systematics’ use of pattern explanations provides the key to answering the doubts about inference to the best explanation with respect to systematics qua historical science.

In section 6.1, I amend and expand Lipton’s (2004) account of abductive inference to show how systematists handle the worry about generating candidate explanations. Yet this shifts the focus to another worry, the problem of evidence, which historically has been a central concern of systematics. Character analysis is the central problem for contemporary systematics, yet the general problem of which it is a case – what counts as evidence? – has not been adequately discussed by proponents of inference to the best explanation. Facing this problem is the price of adopting abductive methods.

In section 6.2, I expand from Whewell's account of consilience to explain how systematists infer tree hypotheses from the convergence of tree topologies hypothesized in context of distinct theoretic domains. This convergence illustrates the use of explanatory virtues in phylogenetic inference apart from consideration of likeliness. Thus, 6.2 shows how systematists handle the tautology objection.

Following Lipton (2004) I use "abductive inference" synonymously with "inference to the best explanation". There is no standardized taxonomy of these terms in the literature, though some authors seek to link distinctions in types of abductive reasoning to terminological distinctions. I seek to clarify the relevant distinctions in-text.

My account concerns broader challenges for abductive inference than the issues targeted by Kirk Fitzhugh (2006, 2008). Fitzhugh's strategy is to claim that phylogenetic inference is essentially abductive, and then argue that various methodological principles must be adopted, altered, or discarded in light of their fit with the abductive structure of systematics. His philosophical account of abductive inference suffers from serious problems and does not provide a helpful starting point for my present purposes. Sober's (1991, 2008) account of common cause arguments in systematics focuses on comparing particular algorithms that are complementary parts of contemporary systematic methodology. My concern here is to situate systematic inference within a more general philosophical account of inference to the best explanation, answering challenges to the legitimacy of inference to the best explanation.

The account I develop complements Carol Cleland's (2002, 2011) arguments that abductive inference is especially useful in the historical sciences. Cleland argued that the inherent epistemic challenges facing historical sciences – the operation of "information-destroying processes" (Sober, 1991; Turner, 2007) – are counter-balanced by the epistemic over-determination of the past by the

present. The key to Cleland's arguments is the availability of multiple, distinct sources of evidence. I here flesh out how such evidence is handled in systematics in particular.

## 6.1. THE UNDERCONSIDERATION OBJECTION AND THE PROBLEM OF EVIDENCE

On some accounts, historically descended from Charles Saunders Peirce, abductive inference primarily concerns the generation of new hypotheses. What I will call (following Lipton 2004) the *underconsideration objection* to abductive inference is already suggested by a puzzle that faces Peirce's classic formulation. Peirce offered a logical scheme (*Writings* vol. 5, p. 189):

The surprising fact, C, is observed.  
But if A were true, C would be a matter of course.  
Hence, there is reason to suspect that A is true.<sup>52</sup>

Harry Frankfurt (1958) noted the tension between Peirce's claims that abduction is a logically formalizable inference that generates new hypotheses and his statements that hypotheses arise via an imaginative faculty that cannot be modeled logically. The problem, Frankfurt argued, stems from the fact that the hypothesis A must already be known before the abductive inference is completed: otherwise A could not occur in a premise of the abductive argument (Frankfurt, 1958, p.

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<sup>52</sup>This formulation captures Peirce's later writings but would not suffice to distinguish abduction from induction in Peirce's earlier writings. In his 5<sup>th</sup> Lowell lecture, Peirce held that both inductive and abductive (hypothetic) inferences enable the reasoner to construct a syllogism which explains the surprising fact of interest. The difference is that inductive reasoning concludes by affirming the rule of the syllogism, whereas abductive reasoning concludes by affirming the case. See for example (*Writings* vol. 1, pp. 427-429).

594). This has been taken by some as a reason to abandon attempts to develop abduction as a logic of discovery.<sup>53</sup>

Other accounts of inference to the best explanation do not claim to yield a logic of discovery. Yet it remains an overarching challenge for proponents of abductive inference to give some account of where explanatory hypotheses come from, with an eye to the underconsideration objection. The underconsideration objection, launched for example by B. C. Van Fraassen (1989, p. 142), is that abduction cannot be a reliable guide to the truth because scientists cannot know that the true explanation is among the candidate pool of potential explanations that they consider. A strong version of the underconsideration objection is that, because there are infinitely many logically possible explanations of the observed phenomena, it is exceedingly unlikely that scientists will have considered the true explanation given finite time.<sup>54</sup>

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<sup>53</sup>See Schaffner (1993) for a brief summary, focusing on Hanson's (1960, 1967) analysis of scientific discovery as largely a matter of abductive (retroductive, in Hanson's terms) inference. Schaffner argued that Hanson did not clearly distinguish between discovery as the initial generation of hypotheses versus a preliminary evaluation stage. Because Hanson did not offer a logic of generation, in Schaffner's view Hanson's abductive inference did not significantly differ from Schaffner's analysis of the process of discovery along the lines of the hypothetico-deductive method. Scientists appeal to the familiar epistemic virtues that employ explanatory criteria in a preliminary evaluation of which hypotheses are plausible, prior to more formally organized testing in a strong evaluation phase.

Schaffner's analysis demonstrates that skepticism about the prospects of abductive inference in filling out a logic of discovery does not entail skepticism about the prospects of developing some other logic of discovery. Schaffner presented a preliminary sketch of such a logic (pp. 8-63). The demonstration was salient because some proponents of abductive inference – notably, Ernan McMullin (McMullin, 1992) – argued that *only* abductive inference enables scientific discovery that is both logically analyzable and genuinely new.

<sup>54</sup>One could ensure that the true explanation is among the set considered by including in the set the hypothesis that all other explanations considered are false (Lipton, 2004, p. 155). The problem is that such a hypothesis would have little to no explanatory power with respect to the contrastive question of interest, and inference to this type of explanatory hypothesis is of no use except perhaps as a starting point for further inquiry.



Lipton's (2004) approach to the underconsideration problem offers a starting point for understanding the generation of systematic hypotheses. Lipton argued that scientists typically formulate problems (areas of investigation for the scientific discipline) in terms of contrastive why-questions. In Lipton's central example, Semmelweis framed his investigation of the cause of childbirth fever as a contrastive question: why is it that women in the First Maternity Division of the hospital suffered a much higher incidence of mortality than did the women in the adjacent Second Division, rather than the same mortality rate? On Lipton's account, scientists generate a pool of hypotheses. Each hypothesis in the pool, if true, would explain the observed phenomena by answering the contrastive question. Scientists infer that the best explanation among the candidate pool is the true explanation and accept that hypothesis into the scientific body of knowledge.

Lipton described the generation of this candidate pool of possible explanations as an investigation into the causal history of the phenomena described in the contrastive question. Having formulated problems in contrastive terms enables scientists to identify test cases in the world: cases where each side of the contrast class occurs, among phenomena with otherwise similar causal histories. In the Semmelweis case, the women in each Division had quite similar causal histories prior to the contrastive state of affairs to be explained. Semmelweis proceeded to investigate the causal histories more closely. Lipton described the investigation as a search for a type of event in the history of the women in the First Division that does not occur in the history of the women in the Second Division, and which satisfies a counterfactual: had the event occurred in the history of women in the Second Division, these women would have shared the same, higher incidence of the fever as the women in the First Division.

Lipton argued that the scientist can engage in empirical manipulations, using J.S. Mill's methods of investigation (Mill, 1843a, p. 450), to determine that the identified counterfactual holds. However, this does not explain how the scientist identified the proposed counterfactual in the first

place. It must occur to Semmelweis to consider the movements of persons entering each Division, framed in terms of their professional responsibilities. Semmelweis must notice the event of women in the First Division being visited by certain persons as an event of the women coming into contact with medical students who perform dissections of cadavers. Only if he considers the contrast in causal histories in this light will he hit upon the contrasting event as counterfactually relevant. Yet there is no guarantee that he will consider the question in the requisite light; the underconsideration objection recurs.

Lipton appealed to background information – the result of prior inductions – as providing general knowledge of what sort of things would support a counterfactual that might explain the contrastive outcome. Semmelweis presumably had background beliefs about the repugnance of corpses such that he would have noticed the appropriate counterfactual. Lipton's response is somewhat vague and it is not immediately evident how it can be applied beyond the Semmelweis case. Phylogenetic systematics shows more precisely how the underconsideration objection can be handled in a successful contemporary science.

In modern systematics the questions of interest are about relationships between taxa. The evidence is characters of some form or another, and the basic task is to propose relationships that explain the observed distribution of characters. Systematists begin with the premises that all organisms are related at some level and that observable characters are inherited, with modifications, within lineages over time. These assumptions, together with phylogenetic methodology (itself based largely upon the assumptions), suffices to identify the necessary set of counterfactual explanations.

Given three organisms, systematists can begin with three phylogenetic possibilities: [[A, B] C] or [[A, C] B] or [[B, C] A].<sup>55</sup>

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<sup>55</sup>This works necessarily for individual organisms and for monophyletic taxa. It does not necessarily work for groups of organisms below the species level, nor for groups of organisms where reticulate inheritance is common (for example, Bacteria and Archaea). For a basic 3-taxon statement

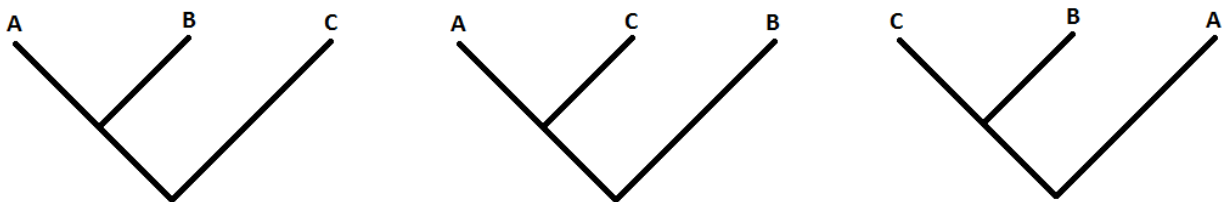


Figure 11. The set of possible 3-taxon bifurcating trees for three organisms designated A, B, and C. The hypothesis at left indicates that A and B are more closely related to each other than either is to C.

Consider the problem of explaining observed characters of the three organisms, such as morphological characters that are shared by A and B, or genetic characters that differ in B versus C. Systematists have three potential patterns of phylogenetic relationship in hand, as depicted in Figure 11. These hypotheses of relationship assert distinct patterns of past history. Each hypothesis constrains what might have been the causal history of the observed organisms. The causal history of the organisms must match one of the three pattern hypotheses, and this pattern is the fundamental basis of the explanation of the observed characters.<sup>56</sup> A, B, and C all share some characters through

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such as I am describing, systematists treat biological entities *as if* they were monophyletic taxa. Over time, in most cases, species will satisfy the logical requirements for 3-taxon statements to obtain: each member of the species is more closely related to the other members of the species than to any non-conspecific organism. See Haber (2012) for discussion of cases in which the degree of genetic relatedness can come apart from ancestral relatedness considered at the level of the individual organism.

<sup>56</sup>There is also a null hypothesis, that the observations do not represent characters of separate, biologically real entities. This null hypothesis covers the logical possibility space in the way considered in footnote 4. The other hypotheses assume that the observed characters, whose distribution is to be explained, are characters of individual, biologically meaningful entities. The assumption is false in the case of an artifact, such as the “type specimen” of Piltdown Man (an artificial association of a doctored human skull and orangutan jaw).

The null hypothesis is also salient when dealing with fragmentary remains which might be assigned to individual organisms. For example, in dealing with a fossil left foot and a fossil right foot, found separately but proximately, paleontologists may attempt to determine whether the remains belonged to the same individual. If so, characters observed from the distinct fossil specimens would be united in a systematic analysis. Alternately, paleontologists might perform

inheritance. If hypothesis  $[[A, B] C]$  is correct, A shares some characters with B but not with C, because those characters arose in the lineage leading to [A, B] after that lineage split from the lineage leading to C. The observed pattern of character distribution would have been different, had the actual pattern of historical descent been different.

In phylogenetic inference of this form, the underconsideration objection can be handled by the automatic generation of all possible patterns of descent. For typical analyses involving very many specimens, specimens may be grouped prior to phylogenetic analysis into populations or other operational units. There is a well-defined role for background knowledge in determining these operational units. When the number of operational units precludes the generation of all possible trees for practical reasons, the underconsideration objection can be met head-on through computational strategies designed to carve up the set of total possible explanations.

However, there is an attendant difficulty. Systematists do not have in hand all possible characters. Systematists have a well-defined set of patterns that can potentially explain character distribution, but infinitely many potential characters can be described. Between any two specimens, infinitely many contrasts can be drawn, and systematists face the challenge of determining which contrasts stand in need of a phylogenetic explanation. This is the problem of evidence: what is the relevant evidence that our hypothesis is to explain?

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separate analyses on the basis of the individual parts – for example, hypothesize trees on the basis of all left foot specimens, ignoring the right feet.

The determination of what counts as an individual organism can be problematic in some taxa, for example with many fungi.

Hennig (1966) identified the ultimate unit of systematic analysis as the semaphorant (an individual organism during a restricted time span, “however brief” – p. 6), but restricts phylogenetic systematics to analysis of phylogenetic relationships that result in patterns of ancestry. Observations taken of the same individual are united so that the individual organism over its lifespan is treated as a single entity in phylogenetic analysis.

Specifying the contrast class requires not only broadly identifying a character, such as the number of stamens of a flower, but specifying the way in which the character state can differ. For example, given two plant specimens, we might seek to explain:

Why does specimen A have three stamens rather than four?

Why does specimen A have an odd number of stamens rather than an even number?

Why does specimen A have fewer stamens than five stamens, rather than five?

Why does specimen A have fewer stamens than does specimen B?

The basic challenge is to decide what characters are to be described and how they should be individuated. Philosophers have pointed out the challenge by asking, what enables us to dispense with trivial characters, from Whewell's (1840b) "has exactly 100 leaves" to MacLaurin and Sterelny's (2008) "distance from Brittany Spears' midriff when she turned 18"? J.S. Mill (1843a, pp. 168-169) suggested as a criterion that we rule out logical expansion of observed characters. If having three stamens is identified as a character, then Mill's criterion rules out the proposed character "has three or 100 stamens", "has three stamens or is brown", etc. But the problem remains: how to identify the initial characters? Why initially pick out the character as having exactly three stamens, rather than one of the formulations listed above?

Explicitly or implicitly, systematists answer this question by appeal to background knowledge drawn from biological theories. Biological background knowledge informs the claim that the number of flower petals is likely to be meaningful for systematic analysis, and that distance from Brittany Spears' midriff is not. Background knowledge also informs the choice of contrast class, as expressed by the way the way the character is picked out.

We saw in chapter 3.2.1 the importance of theoretical background knowledge in fixing terminology in terms of which characters will be described, as exemplified by Candolle's arguments about Adanson's system. The role of background knowledge is made explicit in Candolle's choice of contrast class to describe the number of stamens in Cruciferae (de Candolle, 1813, pp. 121-122). For the most part, plants in the family Cruciferae bear flowers that have 4 petals and 6 stamens.<sup>57</sup> Candolle asked with what other plants do the Crucifères bear the closest affinity? He considered two candidate groupings. The first hypothesis groups the Crucifères with plants that bear flowers that have twice as many stamens as they have petals. The other hypothesis groups the Crucifères with flowers that have an equal number of petals and stamens.

Candolle then speculated as to what causal story would obtain in the case of each grouping. He imagined that if the Crucifères belong in the first grouping, the "primitive state" (*état primitif*) of Crucifères bore flowers with four petals and eight stamens. Two stamens would have been aborted through some developmental process, resulting in the observed Crucifère form.<sup>58</sup> Candolle supposed that if the Crucifères belong in the second proposed grouping, each Crucifère flower would originally have had four petals and four stamens, with flowers growing in clusters of three. On this hypothesis, each group of three flowers would fuse such that the two lateral flowers disappear, leaving behind only a single stamen each.

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<sup>57</sup>Candolle's group, Crucifères, corresponds to the family now called Brassicaceae; this includes mustard, cabbage, water cress, and 338 total genera (Al-Shehbaz, Beilstein, and Kellogg 2006).

<sup>58</sup>Here I (like Whewell – e.g. 1857c, p. 365) use "aborted" for *avorté*; note that avortement does not always involve the complete loss of parts. Candolle's definition: « *Avortement (Abortus)*, état dans lequel un organe n'a pas pris l'accroissement qui lui est ordinaire, sans qu'on puisse toujours y reconnaître une cause immédiate. » (Candolle 1813, p. 406). My translation: "state in which an organ has not attained the growth that is ordinary to it; one is not always able to recognize the immediate cause."

Candolle offered three arguments that the second hypothesis is more likely true. He observed that some species that manifestly belong in Cruciferae (on the basis of many other characters) lack the two lateral stamens. Some Crucifère species that normally have the four petal, six stamen pattern produce abnormal individuals that bear flowers with four petals and four stamens, with the two “missing” lateral stamens having changed into complete flowers. The two lateral stamens in typical Crucifère individuals always attach below the point of attachment of the other stamens and the petals themselves.

Candolle set his contrast class by describing the different possible groupings in terms of a formula of the relative number of petals to stamens (1 petal : 2 stamens, versus 1:1). Candolle asked why specimens of particular plants (Cruciferae) displayed different ratios of petals to stamens. Even though Candolle lacked precise knowledge of the developmental processes through which the number of petals and stamens arise, he reasoned about what in the developmental causal story of the particular plant would have to have been different if the observed character did not obtain.

Note that Candolle’s reasoning is phrased in terms of development. Candolle’s terms “primitive state” and “originally” are not to be understood here as explicitly evolutionary. Candolle used the terms methodologically, in the sense described by Naef (1972, pp. 14-15). Darwin argued that the fact of common descent would explain both the fact of a hierarchical arrangement of organisms and the success of particular systematic methodologies (C. Darwin, 1871, pp. 378-379). Indeed Darwin attributed the success of systematists like Candolle to unconscious application of the idea of common descent (C. Darwin, 1871, p. 382). Darwin made this claim in light of systematists’ adoption of many seemingly distinct guiding principles about the use of rudimentary (vestigial) characters, characters of little physiological importance, correlated characters, adaptive characters, embryological characters, reasoning about intermediate forms, and the genealogical criterion of species. Darwin claimed that only the fact of common descent can explain the success of all the

diverse principles, and only the unconscious recognition of common descent could explain systematists' adoption of these principles. However, systematists might have used these principles because of their success in producing concordant classifications without recognizing (consciously or otherwise) common descent as a unifying, over-arching idea. Candolle's reasoning, as I describe it above, required background knowledge about physical possibility and developmental resources. Candolle did not require the knowledge that these possibilities and resources result from the past operation of evolutionary processes.<sup>59</sup>

To demonstrate the role of background knowledge in contrastive reasoning about character states, consider an alternative account. Can judgments about characters be grounded without appealing to the content of biological theories? While Elliot Sober (1991) recognizes the role of theoretic content in character analysis, he suggests a criterion that might ground an atheoretic account. I will consider his criterion in order to demonstrate that it involves implicit use of background knowledge drawn from theories relevant to the domain of inference.

Sober (1991, pp. 212-225) argues that the evidentiary significance of a shared character is grounded in the character's rareness.<sup>60</sup> Rareness seems to be a pretheoretic criterion; can rareness do the work of specifying a contrast class for abductive inference? The idea is that less common

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<sup>59</sup>Knowledge about evolution may help, however, and is necessary to justify the assumptions systematists now make when using phylogenetic methodology. These assumptions are required to generate the set of patterns of relationship as possible explanations.

<sup>60</sup>Sober's concern is to analyze whether and why systematists should take apomorphic characters to be more informative than plesiomorphic characters. Both critics (Mayr, 1982) and proponents (Niles Eldredge, 1977) of phylogenetic systematics identified the privileging of apomorphic over plesiomorphic similarity as the central insight of Hennig's revolution. In most situations, apomorphic characters are more informative than plesiomorphic characters, but Sober argues that it is the expected rareness of the shared characters rather than their status as derived versus ancestral that makes the evidentiary difference.



character states are in greater need of explanation. A phylogenetic hypothesis is sought that would explain the contrast between having the rare character state versus a more common condition.

Sober introduces his arguments with an analogy:

“Suppose you met two individuals (chosen at random) from the U.S. population, and found that both are named Smith. ...you should take this observation to support the hypothesis that the two individuals are more closely related to each other than might be expected *a priori*. But suppose that two other individuals are sampled, and both are found to be named Quackdoodle. This too confirms the hypothesis of relatedness. The question I now want to raise is this: Would you expect the two Smiths or the two Quackdoodles to be more closely related? Intuition suggests that the Quackdoodles are probably more closely related. A hunch as to why this is so is that *rare characters are better evidence of relatedness than common ones*.” (Sober, 1991, p. 213).

Sober then suggests that some feature about name transmission may underlie the judgment in the Quackdoodle scenario, and this suggestion I here develop. The intuition about the evidentiary significance of the name “Quackdoodle” involves much background knowledge, other than simply an intuitive assessment of the character’s rareness.

Of all the ways in which the observer might consider the two individual persons in question, the observer selects surnames. The choice of character states for consideration, and the way in which they are individuated, already reflect background knowledge about the link between surnames and the question of relatedness. The observer knows that surnames are likely to be relevant, as well as what counts as a name and that the name as a whole is likely to be meaningful while its components (syllables, letters, etc.) are not. All of these assumptions can be violated, and in ways that decouple the surnames’ evidentiary value from their rareness. For example, within a Catholic context, “Cardinal” is part of the name of each appointed cardinal. While the name “Cardinal” is quite rare in the U.S. population, the fact that Donald Cardinal Wuerl and James Cardinal Hickey share this name is not good evidence of their relatedness. To an observer grounded in the Catholic

context, rareness is not considered and plays no role in the intuitive assessment of the character's value.

Those of us thoroughly embedded in the genealogical theory of naming might argue that “Cardinal” doesn’t count as a name, even if it functions the same way as a name in all observable contexts other than the initial moment of appointment. This argument points out that an alternate process of name-giving that is grounded in a religious context does not figure in reasoning or explanations with respect to the genealogical context of name-giving. Other examples can be given to show names that are recognized as proper within the genealogical context, yet which are not good evidence of genealogical descent due to the availability and prevalence of other mechanisms of name acquisition. The name “Singh” is often acquired through genealogical mechanisms and in some contexts is also often acquired through alternative, religious processes. Converts to Sikhism adopt this surname, and in some local contexts, this is the primary mechanism for acquisition of the name “Singh”.

The suggestion that rareness accounts for the intuition about the name “Quackdoodle” relies on the reader’s background knowledge. We are not aware of any processes for acquisition of the name “Quackdoodle” that we think to be good competitors with a genealogical account. Since Sober’s book has been published, one competing account might be that the two individuals had each read Sober’s book and been inspired to change their names to “Quackdoodle”. Our intuitions about the case depend on background knowledge about the inheritance of names and legal and social mechanisms for name changing.

The compelling force behind our intuitions in these cases includes much background knowledge about how *hard* it would be for the world to have produced one versus some other state of affairs. Given two individuals named Quackdoodle, we begin with the assumption that this name was acquired in the genealogical way for one of the individuals. Not much would have to be

different about the world for there to be two surviving members of the same immediate Quackdoodle family, versus a world in which only one Quackdoodle survives. On the other hand, positing that the two Quackdoodles are not closely related to each other requires a greater shift in our assumptions about the world. We must posit the existence of two separate Quackdoodle clans that acquired the name separately, or else the existence of a very old Quackdoodle clan whose surviving members are only distantly related to each other. This requires more than just accepting that the name is more common than we had thought, notwithstanding our failure to have encountered any Quackdoodles previously. Crucially, we judge the two-clan state of affairs to be a greater disturbance of how the world must be because of our understanding of the particular linguistic conventions that we recognize from experience of other names. Though I have never met a “Quin”, I know that there are many Quinn’s and that names can change in such a way that the name Quin would not be difficult to obtain, even if the name is exceedingly rare. Similarly, constructions like MacQuinn or Quinn-Depp would not surprise me. Notice that we have some idea what it would take – what would have to be true about the world – for these names to occur.

The name Quackdoodle is not simply rare. It does not match well with our knowledge of linguistic conventions as they relate to probable processes of name acquisition. The intuition in the Quackdoodle case, and the force of reasoning in judgments about characters, involves much beyond rareness. The intuition is that this particular instance of a rare character is the kind of thing that potentially marks an important divergence from what other circumstances might have obtained.

Systematists identify characters that, in the light of background knowledge from scientific theories, are potentially relevant to understanding the causal history of the specimen or taxon. I am here claiming that character analysis fits the general form of contrastive reasoning about what would have to be different about the world in order that the observed situation not obtain.

Candolle noted that a particular plant bears four stamens and six petals; another bears four stamens and four petals. Character analysis indicates that the contrast class of interest is best framed as a doubling of flower with fusion of stamens, rather than two additions of petal or two reductions of stamen number. Candolle reasoned about ways in which the world could be different, in order to specify what character is in need of explanation in light of what contrasting state of affairs. A systematic explanation would then clarify why certain taxa share the doubled state versus the non-doubled state. In phylogenetic systematics the explanation takes the form of a tree hypothesis with the assertion that the doubled character state arose in an ancestor of the specified clade.

My account runs counter to one strand within (philosophy of) systematics that attempts to isolate observation of data – the generation of evidence – from biological theories. The numerical taxonomists advocated “objectivity” in character selection and weighting in light of a circularity concern (Sokal & Sneath, 1963). Particular hypotheses of relationship should be available to test evolutionary theory, and so, the worry goes, systematists should avoid using evolutionary theory content in generating evidence for systematic hypotheses. Instead, systematists might take measurements according to a standardized list of morphometric characters (maximum skull length; maximum palatal shelf width; etc.), or count up base pair differences, or some other method that lacks explicit causal analysis.

Arguments for “objective” character discrimination and weighting are at least as old as Adanson (1763),<sup>61</sup> and responses are at least as old as Candolle (1813, p. 71). Even if it were possible to individuate characters absent some theoretic context, the choice to weight each character equally

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<sup>61</sup>Adanson did not in fact rely on the calculational method that Sneath (1964) ascribed to him, but rather appealed to comparative methods after already having proposed natural groupings of his specimens. Adanson thus does not exemplify a purely numerical, theory-free approach. The historiographic error about Adanson’s methodology appears to trace back through Candolle to Cuvier.

would itself be theory-laden. Further, choices about character individuation necessarily affect character weighting, and character individuation is steeped in theoretic considerations. A trivial example is the proposed character “striped tail”, which might be broken down into multiple characters for each stripe or for subsets of the total number of stripes. Ruse’s (1988) example is counting hair color en masse versus counting the color of each hair at a time. In point of fact, theoretic considerations informed the choice of which characters are to be included in now-standardized lists, and what types of observations can count as characters. Even if present choice of characters is made without deliberate theoretic consideration, the outcome of systematists’ choices will reflect the theoretic considerations that went into the development of methodologies for identifying characters. The morphological character lists that have come to be recognized as standard, and which may now be scored absent explicit justification, were developed through a long and successful history of systematic work. If systematists fail to develop explicit theoretic justification for the use of standardized sets of measurements, nonetheless theoretic considerations led to the development of these standardized sets.

In practice, systematists regularly do focus on character analysis, explicitly marshalling theoretic resources to assess the meaningfulness and evidentiary status (phylogenetic signal) of proposed characters. Some biologists (e.g., Rieppel & Kearney, 2006; Wheeler, 2007) and philosophers (Richards, 2003; Winther, 2009) have recently criticized a contrary trend toward the uncritical use of observable features of organisms without attention to their biological meaningfulness.

To sum up my account of systematic inference so far, let us return to Lipton’s account of abductive inference. Lipton describes scientists as investigating the causal history of events to be explained, searching for a crucial event framed in contrastive terms. If the key past event had not occurred, the event to be explained would not have occurred, but instead an event of an identified

contrasting type would have occurred. Systematic inference proceeds in terms of characters, patterns of characters, and hypothesized patterns of relationships among lineages, rather than identified past events. The patterns that are inferred will be hypotheses of the form described in chapter 5, and are explanatory of the observed character patterns. The hypothesized pattern of history explains why the observed pattern of characters, rather than some other pattern, occurs. The identification of characters requires background knowledge to specify a contrast class in terms of which the character is considered. The contrast class in which the character is considered matches up to the contrast class of phylogenetic explanation. The possible hypotheses of relationship fall out naturally from phylogenetic methodology, on assumptions following from the theory of descent with modification (that organisms share common descent, and characters evolve). Systematists then decide which pattern hypothesis best explains the evidence, that is, the character data.

Both systematists and philosophers of biology have debated the choice of algorithm used to evaluate tree hypotheses in light of character data. Farris (1983) laid out the terms of debate so: “phylogenetic theories are chosen, just as any scientific theory is, for their ability to explain available observations. I shall thus concentrate on evaluating proposed methods of phylogenetic analysis on that basis.” The debates focus on the formalization and justification for application of explanatory criteria.<sup>62</sup> One criterion for assessing which pattern hypothesis best explains the observed distribution of characters is simplicity. Essentially, the idea is to posit the simplest pattern of lineage splitting and character acquisition. The shape of debate about the justification of a simplicity criterion will be familiar to philosophers who address the role of the explanatory virtues in science in

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<sup>62</sup>The debate in systematics literature often takes the form of parsimony versus maximum likelihood and Bayesian approaches. In this context of systematics literature, “parsimony” refers to a technical term for a particular set of methodologies - algorithms that formalize the simplicity idea. Maximum likelihood and Bayesian approaches provide different ways to assess which tree best explains the available data. Disagreement between approaches is about how this determination ought to be made. I take no stand about the relative advantage of any particular approach.

general: does the use of simplicity in scientific inference presume that the world operates parsimoniously?<sup>63</sup>

Regardless of its justification, and setting aside the debates on formalization, the use of an explanatory criterion follows from systematists' concern that the selected phylogenetic hypothesis is the best explanation of the observed character set. Simplicity underlies contrastive reasoning about ways in which the world could be. A state of affairs that is simpler is in some way easier to get to, more likely to have obtained, than some more complex way or having come about.

In the next section I describe how systematists use the results of the work described in this section, which proceeds within individual theoretic contexts, to support pattern hypotheses that are shared across multiple theoretic contexts. This further step also represents abductive inference, using a form of *consilience*. My aim in explicating this stage will be to show how systematic inference handles the tautology problem that has troubled accounts of inference to the best explanation.

## 6.2. CONSILIENCE AND THE TAUTOLOGY OBJECTION

Systematists form explanatory hypotheses that are shared across multiple distinct theoretic domains – the domains of molecular, developmental, and evolutionary biology, ecology, biogeography, and paleontology. A shared tree hypothesis is taken to explain the evidence that is separately provided from each domain, with background knowledge from each domain helping to frame the evidence (section 1). The disparate domains work with different conceptual structures and causal ontologies,

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<sup>63</sup>See Ereshefsky (2001) and Sober (1991) for discussion of the general philosophical problem, using the example of systematics. See Schuh and Brower (2009) for recent, forceful argument that parsimony methodology in systematics does not presuppose that evolution operates parsimoniously.

and we can understand the shared tree hypothesis as remarkably indeterminate about causal ontology. The present section accounts for how systematists infer from quite distinct types of evidence a single, ontologically indeterminate explanation.

The fact of agreement is itself evidentiary and provides a response to what Lipton (2004) describes as the *tautology* problem for inference to the best explanation. The worry is that, if what makes an explanation the best is simply that it is judged most probably true, then abductive inference trivially concludes that the explanation that is most probably true is most probably true. For an account of inference to the best explanation to have substance, the account must explicate the best explanation as the "loveliest" explanation in terms of explanatory virtues apart from direct judgment of probability. In this section I argue that systematics has at least one way to resolve the tautology problem, and that is via consilience.

Consilience is more than simply the agreement of more and more data. The concept of consilience I have in mind appeals to explanatory virtues. Addition of data, including data of different types, need not in itself lead to the conclusion that a hypothesis has explanatory merits beyond its probable truth. To resolve the tautology, the concept of consilience must address the relation of scientific theories to data and to proposed explanatory hypotheses.

Several distinct versions of consilience can be drawn from Whewell's work. Whewell describes consilience as "one of the most decisive characteristics of a true theory" (Whewell, 1840b, p. 446), and consilience may be viewed as a verification criterion. Consilience is a test of a hypothesis. Passing this test is a remarkable feat that can be explained by the truth of the shared hypothesis. Thus consilience can provide the basis for an inference to the best explanation. However, Whewell was clear that the test compares hypotheses that were developed through the



inductive process. Consilience is more than simply the agreement of different pieces of evidence with the proposed hypothesis; the agreement is between distinct lines of reasoning.<sup>64</sup>

Moreover, we must be careful not to view Whewell's consilience as a kind of after-the-fact test to be passed after the hard part of scientific inference is completed. Considered in light of the Whewell's claims about the fit between a theory's concepts, data, and hypotheses, consilience plays a central role in scientific progress.<sup>65</sup>

Whewell described the role of consilience in scientific progress in volume 2 of the *Philosophy of the Inductive Sciences*. He first described the unique power of consilience as a guide to truth:

“But the evidence in favour of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. ... No accident could give rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, when the agreement was unforeseen and un contemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides.

Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions*.” (Whewell, 1847b, p. 65)

This version of consilience describes the fit between a single hypothesis and lines of reasoning within fundamentally distinct theoretic domains – domains that are organized under

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<sup>64</sup>Snyder (2006, p. 175) claimed that, because it requires convergence of distinct lines of evidence, Whewell's consilience is distinct from inference to the best explanation. Snyder's argument presumes that in abductive inference a causal law is “postulated merely because it explains or accounts for these different classes of facts”; the causal law is “imposed from above as a means of tying together different event kinds.” As was seen in section 6.1, more fully developed accounts of abductive inference recognize that considerable work goes in to generating candidate explanations (hypotheses, sometimes including proposed causal laws). Realistic accounts of abductive inference in science situate the inference within a complex, iterative process of abductive, inductive, and deductive reasoning.

<sup>65</sup>See Laudan (1971) for historiographic argument for the importance of consilience in the development of Whewell's philosophy of science.

wholly different conceptual structures. Whewell's primary example of consilience was Newton's discovery of the inverse square law of gravity, a new Law of Causes (chapter 3.1.2). Newton's hypothesis would not only explain all three of Kepler's Laws, but would also account for the precession of the equinoxes (Whewell, 1847b, p. 66) and the tides (Whewell, 1837a, pp. 246-247). As Laudan puts it, the great strength of consilience is showing that apparently distinct sets of facts are explained by the same fact about the world (Laudan, 1971). All Whewellian inductions involve the matching of a new concept to the phenomena of experience (Whewell, 1847b, p. 77). In the strong form, consilience results in the discovery of a concept that groups phenomena previously understood separately under a new, unifying concept. The moon, earth, sun, planets, and terrestrial objects including water are all "objects with mass" – that is, objects subject to the inverse-square law. This form of consilience thus includes both of the epistemic virtues Magnus (1996) distinguishes as "consilience" (the agreement of distinct lines of evidence) and "breadth" (the ability of a theory to account for a broad range of phenomena).

The strong version of consilience describes not only agreement of distinct theories about particular cases, but a coming together of the conceptual structures of distinct theories. The expansion of the unified conceptual framework supports inductions of much greater generality, which encompass the narrower generalizations that historically preceded them. In Whewell's view, this is constitutive of the progress of science (Whewell, 1847b, pp. 73-74; 77-78).

Ernan McMullin (1992) agreed that this strong form of consilience is fundamental to scientific progress. McMullin argued that Whewell's consilience is the key that "makes science" because it provides new access to the causal structure of the world. The realization is that apparently distinct phenomena are in fact related through the discovery about the causal structure. The discovery is a unification of distinct causal concepts and so requires agreement about causal ontology. Whewell evidently foresaw such a unification on the horizon for botany and physiology as

botanists came to explicitly recognize and develop the Fundamental Idea of Final Cause, previously the concern of physiologists (chapter 3.2.3).

However, there is a more localized version of consilience that does not require merging distinct disciplines, or the radical expansion of conceptual framework through recognition of the overlap of previously distinct concepts. Ruse (1979) identified Whewell's natural affinity criterion (chapter 3.2) as a local equivalent of consilience in the context of systematics.<sup>66</sup> In Ruse's view, natural classificatory hypotheses are fundamentally distinct from scientific theories because the latter make claims that have a higher level of generality and also some kind of necessity (Ruse, 1979, p. 532). For Ruse, consilience in systematics must be of a different kind than consilience across theories. In light of the arguments of chapters 4 and 5, it should be clear that historical systematics makes claims on the world that invoke causal necessity. The difference between Ruse's theoretic consilience and the consilience criterion in systematics comes to the level of generality, which may have consequences for the progress and course of scientific disciplines.

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<sup>66</sup>D. Magnus (1996) examined epistemological debates among early twentieth century taxonomists and claimed: "While the naturalists in this debate did not use the term consilience, there is a causal chain from the early development of the concept by the early 19<sup>th</sup> century philosopher of science William Whewell, and the neo-Darwinian natural historians." His case rests on an analysis of the debate between natural taxonomists (J.T. Gulick, D.S. Jordan, C.H. Merriam, and A.E. Ortmann) and experimentalists (C.S. Gager and D.T. MacDougal). In this dispute, Magnus argued that the natural taxonomists' appeal to "the Darwinian Method" was an appeal to a picture of science descended from Whewell. Whereas the natural taxonomists sought consilience of lines of evidence, and valued the breadth and convergence of theories, the experimentalists stressed replicability and inter-subjectivity objectivity.

Henson (1990, 1993) argued that J.H. Comstock, an early 19<sup>th</sup> century entomologist, introduced consilience to systematic practice. Henson's arguments focus on the convergence of distinct lines of evidence rather than consilience across conceptual frameworks.

Magnus also claimed that Whewell's consilience was a central influence on Darwin. His case for this claim rests on Ruse (1975) and Thagard (1977) (see page 158).

The local version of consilience requires agreement on hypotheses that support explanations of particular problems, without substantial realignment of conceptual structures. The new concepts produced in local consilience are about the local phenomena. The scientist has successfully latched on to something about the causal structure of the phenomena in question, but need not have a detailed understanding of that causal structure. In particular the scientist need not develop a causal ontology that holds across distinct domains of inquiry, as is required in the case of global consilience. Candolle's recognition that the *Acotylédoné* versus *Cotylédéon* distinction lines up with the distinction between vascular and cellular plants did not require a shared ontology between study of nutritive function and reproductive function (3.2.2).

In both forms of consilience there is a convergence of distinct lines of reasoning that are connected to partially distinct conceptual frameworks. The important common element is the justification for the use of consilience. It is the explanatory virtues of the shared hypothesis that support the conclusion. The hypothesis is seen to have great unity and simplicity (Whewell, 1847b, pp. 70, 74). The hypothesis has great predictive power and most especially predictive power across domains (scope) (Laudan, 1971, p. 373; Whewell, 1847b, p. 65). In the case of natural affinity, natural classificatory hypotheses are seen to explain the distribution of characters, including newly observed characters and especially characters of different form. Natural classificatory hypotheses support generalizations about the morphology, geography, development, and ecology of classified entities.

The explanatory value of consilience derives from its connection to partially distinct conceptual frameworks. As McMullin (1992) argued, laws do not explain but only describe regularities; theories explain, and theories are much more than collections of observed regularities. Collecting data and applying laws about regularities does not suffice to provide hypotheses with explanatory virtues, but at best delivers some kind of probability measure. The loveliness of a

proposed hypothesis derives from the alignment of the hypothesis and observation with the conceptual structure of a theory about the world. The driving force of consilience is an inference that the truth of the shared hypothesis is the best explanation of the alignment across distinct frameworks.

Modern systematics draws on the consilience of lines of reasoning drawn from distinct theoretic domains. Abductive inference in systematics thus relies on the explanatory virtues that justify consilience, and is not simply a direct assessment of the comparative probability of distinct explanations. In systematics, consilience supports the adoption of an explanatory hypothesis about pattern. The pattern hypothesis is shared across multiple domains, and draws support from the fact of its explanatory role within each theoretic domain. Abductive inference provides causal knowledge in the form of facts about the pattern of tree topology.

Facts about tree topology take various forms. To illustrate topological facts and phylogenetic inference, I here present data analyses from a recent systematic revision of the genus *Bassaricyon* (Helgen et al., 2013). I focus on the study's consilience of morphological, morphometric, and molecular genetic lines of reasoning; the study also included ecological and biogeographic analyses whose results were in accordance with the hypothesized tree topology.

The authors described morphology of 115 specimens. Morphological similarities and differences were used to group specimens. These groups were hypothesized to represent clades (four species were designated in the final taxonomy; four sub-species of *B. neblina* and two sub-species of *B. medius* were also designated). The study authors used a standardized set of characters and protocols to obtain skull measurements. A principle component analysis was performed on

morphometric data and the results were used to refine and support the grouping hypotheses (Figure 12).

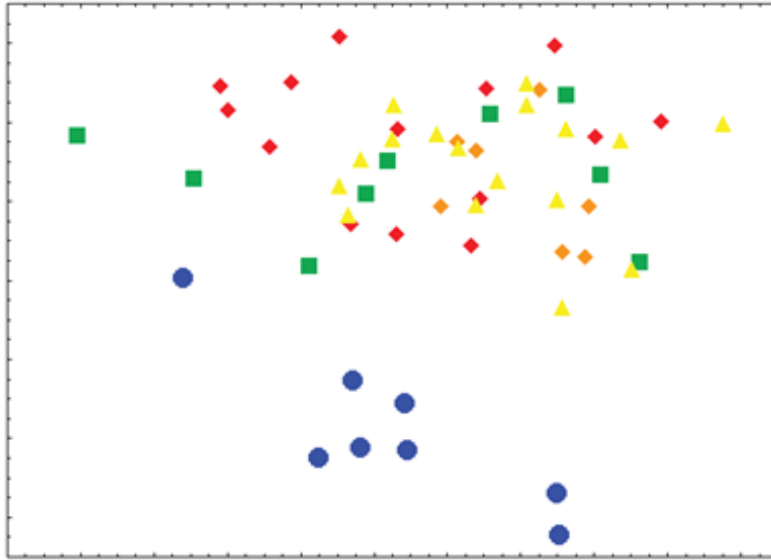


Figure 12. Morphometric analysis of 55 specimens referred to *B. neblina* (blue circles), *B. gabbi* (green squares), *B. alleni* (yellow triangles), *B. medius medius* (orange diamonds), and *B. medius orinomous* (red diamonds). Distance between dots (specimens) indicates morphological distance calculated on the basis of 24 craniodental measurements. All specimens represent female individuals; males specimens display a similar dispersion pattern. The sexes were analyzed separately because the genus exhibits sexual dimorphism. Assignments to species were made on the basis of discrete morphological characteristics of skull and body. Reproduced from (Helgen et al., 2013).

The specimens represented by blue circles in Figure 12 are separated in morphospace from the other specimens. In addition to the clustering in morphometric analysis, the specimens could be reliably diagnosed as a distinct group on the basis of skull and body morphology. The study authors ultimately referred these specimens to a newly proposed taxon, *Bassaricyon neblina*. The morphological uniqueness of these specimens suggests the hypothesis that *B. neblina* is sister to all other *Bassaricyon*.

This hypothesis could be further supported through character analysis to identify derived character states relative to a known outgroup, such as *Nasua* (page 151).

The clustering of other *Bassaricyon* specimens becomes evident when specimens referred to *B. neblina* are removed from the data set, and the data is evaluated at a finer scale Figure 13.

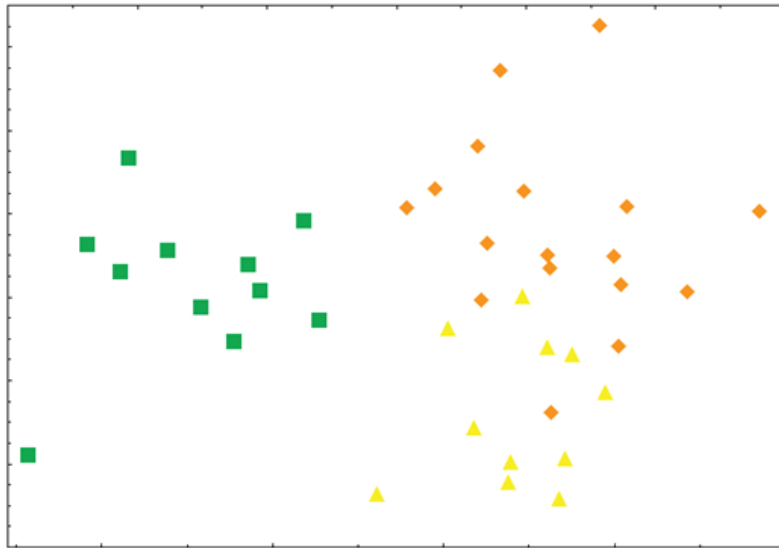


Figure 13. Morphometric analysis of specimens referred to *Bassaricyon*, excluding specimens referred to *B. neblina*. Dots represent 39 adult male olingo skulls, plotted using 8 craniodental measurements, referred to *B. gabbi* (green squares), *B. medius* (orange diamonds), and *B. alleni* (yellow triangles). Reproduced from (Helgen et al., 2013).

The authors sampled two genes (cytochrome *b* and *CHRNA1*) from ten of the same specimens. The genes (separately, and concatenated) were analyzed using parsimony, maximum likelihood, and Bayesian approaches. The maximum likelihood analysis yielded the tree depicted in Figure 14.

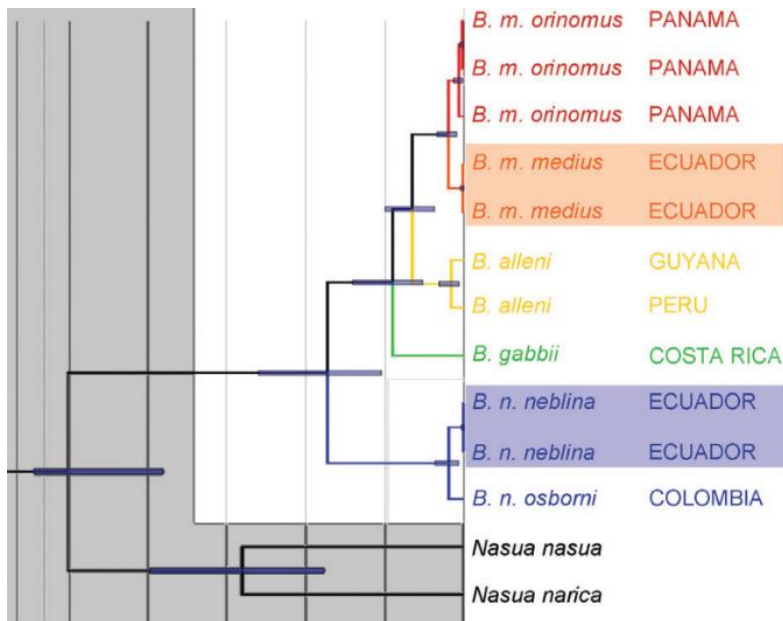


Figure 14. Maximum likelihood analysis of relationships in the clade that includes *Bassaricyon* and *Nasua*. Reproduced from (Helgen et al., 2013).

The tree presents a hypothesized series of lineage splits and changes in genetic characters.

The phylogenetic hypothesis is the order of branching of distinct lineages of organisms that bear the genetic characters. The tree also includes range estimates of when the branch splits occurred.<sup>67</sup>

<sup>67</sup>Date range estimates were compared with estimated date ranges of historical geological and biogeographical processes, such as the formation of the Panamanian isthmus and the subsequent Great American Biotic Interchange. Present geographic distributions of proposed taxa were estimated through bioclimatic range modeling and used to form hypotheses of historical dispersals. Hypothesized historical barriers to dispersal constrain the dates of lineage branch splits. (Helgen et al., 2013)



Helgen et al. (2013) used the results of the above analyses to support the tree hypothesis of Figure 15.

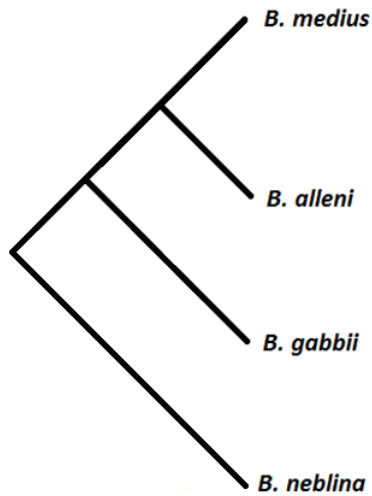


Figure 15. Relationships of *Bassaricyon* species.

The morphological and morphometric analyses supported grouping specimens into the four depicted lineages. Morphological and morphometric analyses also supported the clustering of some specimens, those labelled *B. neblina*, as a clade that is the sister of all other *Bassaricyon*. These analyses begin with measures of similarity that are, *ceteris paribus*, taken to be evidence of genealogical relationship. The authors identified characters that diagnose clades within *Bassaricyon*. These diagnostic characters could be further explored through explicit character analysis with the goal of supporting hypotheses of homology and character polarization. A variety of criteria are used to identify parts as homologs, that is, copies of the same part inherited from a common ancestor (N. Eldredge & Cracraft, 1980, pp. 53-85; Patterson, 1982; Remane, 1956; Wiley & Lieberman, 2011, pp. 122-137). Characters of these parts are compared across proposed groups. Character polarization is the determination of which character state is plesiomorphic versus apomorphic, and is achieved

through outgroup analysis (see Schwartz, 2009; and Schwartz, 2011 for criticism of faulty criteria and assumptions widely used in assigning character polarity). Group hypotheses would be supported by the finding that morphological similarities used to support the groups are apomorphic. Placement of *B. neblina* as sister to other *Bassaricyon* would be supported by a nested pattern of apomorphic characters that matches the proposed topology.

Morphological evidence is evaluated in the context of developmental biological knowledge. Systematists working in this conceptual domain reason in terms of anatomical, physiological, and developmental structures and processes. The morphologist has knowledge of how “hard” it is for changes to occur, and so has a grasp on the meaningfulness of characters observed in distinct specimens and clusters of specimens (page 136). In the study at hand the theoretical reasoning about morphological character state changes was largely implicit (as discussed on page 139).

The maximum likelihood genetic analysis proceeded in the conceptual framework of population genetics. A model of base pair change was applied to genetic data in order to estimate the tree that renders the observed genetic data most probable. The tree, as depicted in Figure 14, groups specimens into hypothesized clades and hypothesizes the phylogenetic relationships between clades. This method of analysis also hypothesizes branch lengths, that is, the length of time between lineage splits. Branch lengths are not themselves hypotheses of phylogeny (Wiley & Lieberman, 2011, p. 226). The phylogenetic hypotheses are the tree topology and the identification of groups of specimens as species. The molecular analyses generated species hypotheses that converge with hypotheses of the morphological and morphometric analyses. Additionally, the hypothesized tree topology of molecular and morphological analyses converged to identify *B. neblina* as sister to other *Bassaricyon*.

The role of theoretical concepts seems more obvious in the molecular analyses because genetic data is not directly observed in the way that morphological structures can be seen by eye. It

must be remembered that considerable background knowledge is involved in seeing a part as the part that it is. Indeed, arguments about homologous part identification are common and critically important. Character changes must be identified with respect to a frame of reference that requires identification of homology. For example, in comparing craniodental morphology of *Gigantura*, *Trachinocephalus*, and *Bathysaurus* (deep-sea fishes of family Giganturidae), the same observed element might be identified as a remnant of the maxilla bone (Walters, 1961, p. 313); the second infraorbital bone (Rosen, 1973, pp. 438-441); a dermal ossification; or an observational error, such as a cheek scale mistaken for a bony element on a microscope slide.<sup>68</sup> Identification of the part is crucial to argumentation about proposed character state changes across these taxa, which in turn is crucial to phylogenetic inference. In the example case, authors' phylogenetic hypotheses depended on their interpretation of the observed element.

The fact that there is morphological uniqueness of *B. neblina* turns out to follow from a phylogenetic hypothesis: that the lineage leading to *B. neblina* lineage diverged earliest within the *Bassaricyon* clade. Observations of molecular genetic data are explained by the same phylogenetic hypothesis. The distinct lines of reasoning, operating in distinct conceptual frameworks, converge on the hypothesized tree topology. Consilience is local to the tree topology (indeed, to specific parts of the topology). The consilience does not involve restructuring the conceptual frameworks of the distinct disciplines of molecular biology, comparative anatomy, and developmental biology.

Within each domain, the observed evidence would be explained by the tree topology. Moreover, the fact of the convergence would be explained by the truth of the tree topology. That lines of reasoning in distinct scientific domains all yield a single tree topology would be a surprising fact, except that this topology describes the pattern of history of the observed biological entities.

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<sup>68</sup>See (Konstantinidis & Johnson, Forthcoming) for discussion.

Consilience is a mark that the shared hypothesis is the best explanation by virtue of its scope, simplicity, and explanatory power with respect to multiple domains. The ability of the shared hypothesis to serve in explanations with different targets, across domains, is taken as a mark of the truth of the hypothesis. Consilience across scientific domains is evidentiary because of explanatory virtues, not directly by appeal to probabilities. This type of abductive inference is not tautological in the way that Lipton (2004) framed as a chief concern for accounts of inference to the best explanation. It does not make sense to add up evidence across distinct conceptual domains into a direct assessment of probability. It is the explanatory value of the tree topology across domains that drives the inference.

A potential difficulty with my account is that there are infinitely many potential theoretical domains that might hypothesize facts about tree topology. Some of these hypotheses are manifestly irrelevant, and agreement with hypothesized trees should not be taken as evidentiary. For example, a lexicographical analysis of *Bassaricyon* species names might be proposed as supporting the topology of Figure 5.6. “*B. neblina*” is the only name taken from Spanish, and does not honor a scientist (as do *B. alleni* and *B. gabbi*), and so appears to be the most distinctive taxon name. This is, once again, the problem of discriminating irrelevant targets of explanation (page 130).

Systematists only take note of convergence between pattern facts hypothesized by disciplines that take themselves to be providing evidence of the natural biological classification. We do not expect that patterns of lexicographical similarity will be explained by a natural classificatory hypothesis; such explanations would require radical revision of our understanding of the nature of the natural classification and the process of naming taxa. In some cases, irrelevant matches will be seen to be irrelevant by their own lights. The proposed lexicographic similarity can be evaluated by consulting the protocols for naming taxa (International Commission on Zoological Nomenclature,

1999). In these protocols there is no connection between lexicographic characters and phylogenetic relationship.<sup>69</sup>

Tree hypotheses are only explanatory of character evidence that is appropriately hooked up within each domain (molecular biological, comparative anatomical, biogeographic) to knowledge about the relevant structure of the biological world. By requiring this connection, we can rule out the overarching consilience of tree topology facts from irrelevant domains. The tree topology does a terrible job explaining the lexicographic observations because there is no substantive connection to a theory about name giving. Irrelevant convergence is not likely to contribute to hypotheses with significant explanatory value. In those cases where the irrelevant evidence converges with relevant evidence, the irrelevant evidence does not contribute to the explanatory power of the shared hypothesis. The irrelevant evidence does a poor job making predictions on its own. Assessing the relevance of proposed instances of consilience is part of the work of science to constantly build, test, and reform the connection between conceptual framework, hypothesis, and evidence.

Thus far I have discussed the ideal situation that evidence from distinct, relevant analyses agrees. If hypotheses about tree topology do not converge upon a single tree hypothesis, systematists are left with a puzzle that needs to be resolved. The fact of disagreement is itself viewed as a target for explanation.

Disagreement can be resolved by identifying a source of error in an individual analysis, perhaps by revising background knowledge within the relevant domain. For example, disagreement

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<sup>69</sup>Linguistic conventions that connect to taxonomic “level” have been criticized because they tend to promote erroneous biological assumptions (Ereshefsky, 2001, pp. 214-221). For example the ending “-idae” within mammals indicate a taxonomic family; it is easy then to assume uncritically that Procyonidae is biologically comparable to Daubentonidae, in terms of number of species circumscribed, depth of time, and so on. These are not themselves phylogenetic assumptions but they connect to phylogenetic claims. Linguistic conventions could be used for these purposes. As it stands, the problem is that it is not explicit what claims are intended, and different workers may make different assumptions.

between molecular and morphological analyses has at times led to hypotheses of adaptive convergence; putatively synapomorphic character states are reinterpreted as homoplasious. Disagreement has also led to revision of molecular analyses and models. Morphological studies such as Gauthier, Kluge, and Rowe (1988) support the phylogenetic hypothesis [mammals [birds, crocodiles]], while molecular analyses initially supported the claim [crocodiles [mammals, birds]] (Hedges, Moberg, & Maxon, 1990). Xia, Xie, and Kjer (2003) resolved the disagreement by adjusting the alignment of the gene sequences (18S rRNA) that had been used in the molecular analysis. The case demonstrated the need to consider secondary sequence structure when performing sequence alignment.

When a systematist argues that some data type is unreliable, she is offering a proposed explanation in need of support in each instance in which it is raised. This support is not infrequently offered, framed in terms of the phylogenetic signal of some character sets (for example, Naylor & Adams, 2001; O'Leary, Gatesy, & Novacek, 2003).

In some cases, systematists “bite the bullet” and accept that the explanation is less satisfactory with respect to one or more domains, though more satisfying overall. Most commonly disagreement is not radical. It may be that the pattern best supported by one domain corresponds to the “second choice” of some other domain; or that the pattern hypotheses of the second domain can accommodate the overall pattern with some plausible modification. Decisions about fitting pattern hypotheses may be made on the basis of the scope, simplicity, and explanatory power of hypothesized trees, and rational disagreement is possible. The possibility of disagreement should be understood as a descriptive strength of my account of abductive inference, when the disagreement can be captured in these terms.

Negotiation of disagreement illustrates a feature of abductive inference that McMullin, Lipton, and I agree on: abductive inference is part of a larger scheme of scientific inference. Much

inductive reasoning goes into character analysis, fine-tuning the constraints of phylogenetic methodology (for example determining how much reticulation across tree branches is possible), and choice of tree-building algorithm. In turn, successful abduction to shared tree hypotheses leads to revision of background knowledge going forward.

When claims about tree topology conflict, systematists typically want to know why. Systematists display an overarching concern not just to capture the correct facts about the relationships of organisms. Systematists also are concerned to find the correct explanatory fit between observed data, hypothesis, and the world. My account puts this descriptive into normative context: this is the shape of inference in successful systematics.

I have developed this account of abductive inference in systematics, addressing two key challenges for abductive inference, with the goal of understanding systematics. Peirce, McMullin (1992), and Lipton (2004) argued that abductive inference is crucial in science, and that an account of inference to the best explanation is essential to understanding science. This claim bears out in the case of character analysis and consilience in systematics. My account relies on Whewell's consilience and the account of historical science developed in chapter 5. This stands in contrast to the details of Lipton's account of contrastive causal investigation, which appeal to Mill's methods in an event-network ontology. Whewell's "hard part" criticism of Mill's approach recurs as a problem for Lipton's account and underlies the inadequacy of Sober's argument to ground character analysis on a rareness criterion.

## 7.0 CONCLUSION

In this dissertation I developed an account of systematics qua historical science. I argued against common but flawed assumptions about how causal explanation must work, derived from J.S. Mill. From Whewell I drew an alternative account of historical causal reasoning that stresses the inter-theoretic nature of historical claims in systematics. Modern phylogenetic methods formalize this historical causal reasoning in the context of reconstructing relationships between organisms. I argued that modern systematics employs inference to the best explanation to achieve this task.

Further work is required to relate particular parametric methods in systematics to philosophical accounts of abductive inference. Sober's (1991) invaluable account needs to be updated; his recent (2008) text on phylogenetic inference is helpful but does not discuss the most recent technical methods in depth. Some of these methods partition and search through a space of candidate phylogenetic hypotheses, which on my account are to be understood as candidate explanations. Further philosophical work on these strategies may prove helpful in addressing the bad lot objection in other scientific domains.

Darwin wrote of Whewell: "Next to Sir J. Mackintosh he was the best converser on grave subjects to whom I ever listened." (Charles; Darwin & Barlow, 1969, p. 66). Evidently, many of these conversations occurred as the two walked home from J. S. Henslow's weekly scientific gatherings. Additionally, Whewell and Darwin would have met regularly through the Geological Society from 1837 to 1838 (Ruse, 1975, p. 165). Whewell was invited to join Strickland and Darwin



on the committee that British Association for the Advancement of Science commissioned to reform zoological nomenclature, but Whewell declined (Winsor, 2015).

Sometime in or after 1841 Darwin wrote: “From Herschel’s Review Quart. June 41 I see I MUST STUDY Whewell on Philosophy of Science.” (Ruse, 1975, p. 166). There is scant but suggestive evidence that Darwin in fact read Whewell’s *Philosophy of the Inductive Sciences*. Thagard (1977) argued that Darwin did, pointing to a line in Darwin’s *Variation of Plants and Animals under Domestication* (1868, p. 357): “As Whewell, the historian of the inductive sciences remarks: - “Hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of error.”” Thagard stated that the quote matches Whewell’s *Philosophy of the Inductive Sciences* (Whewell, 1847b, p. 60) except for Darwin’s capitalization of ‘Hypothesis’, Whewell’s spelling of errour, and Whewell’s use of italics (Thagard, 1977, p. 353). In fact, Whewell’s first edition matches Darwin’s quotation on the latter two points (Whewell, 1840b, p. 225). Presumably Darwin capitalized “Hypothesis” because he omitted the first half of Whewell’s sentence. Thagard also argued that Darwin expressed views similar to Whewell’s on the issues of true causes and consilience. Thagard’s description of these concepts is very brief, and further work is needed to assess the nature and extent of Whewell’s influence on Darwin.

Darwin did read Whewell’s (1837c) *History of the Inductive Sciences*, where he would have found Whewell’s account of the nature and progress of historical science. O’Hara (1997, p. 327) has claimed that Darwin’s *Origin* is “almost a casebook of the palætiological principles that Whewell had outlined.” Further historical research is needed to specify and substantiate the idea that Darwin saw himself as applying Whewell’s philosophy of historical science in the domain of biology. Prerequisite to this research, the present dissertation has clarified Whewell’s account of historical science. With its central focus on the limits of presently operating causes, Darwin’s *Variation* reads like a casebook of Whewell’s account of the ætiological component of historical science.

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